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ANNALS

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ERRATA IN VOL. XI.

Page 276. The words of "No. 69—70, in rhomboids," belong to No. 74—75, and must read thus: "massive, and in rhomboids, with copper pyrites."

391, line 16, *for* American, *read* Amician.

// ANNALS

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JANUARY, 1826.

Article I.

Some Observations on the Review of Dr. Thomson's "Attempt to establish the First Principles of Chemistry by Experiment," in the 39th Number of the Journal of Science. By the Author of that Work.

This is the fourth personal attack upon the Professor of Chemistry in Glasgow College which has appeared in Mr. Brande's Journal—all of them from the pen of the same individual, Dr. Andrew Ure, of the Andersonian Institution, Glasgow; who it seems is not satisfied with puffing his own works in all the Newspapers, and all the Reviews and Journals, where he can get a quack-bill admitted; but thinks it necessary likewise to exert his energies to bring down other chemical writers to his own level. Judging, naturally enough, that if in other respects they were all upon a par in the estimation of the public, the most impudent and the most profligate would have the greatest chance of rising into notice. Dr. Henry and myself happening to be the most successful authors of chemical treatises in Great Britain, he levelled his formidable batteries in succession against us both, with what success I leave the public to determine.

To his attacks upon the fifth edition of my System of Chemistry, in which he informs his readers that the book is of no value, but that no chemist can possibly do without it, I made no reply; leaving the two propositions which constituted the main object of the review to refute one another. While engaged in drawing up the review of the sixth edition of my System, he was frequently coming about me, sometimes visiting me in my laboratory, and sometimes at my own house; and were I to repeat the adulation with which he was perpetually loading me, and to compare it with the terms in which he speaks of me in his Review, written at the time when the expressions of adulation were still in his mouth, no one could believe that what I stated

could possibly be conformable to truth; except indeed those individuals who are personally acquainted with the Andersonian Professor. This review was published above a year before I had an opportunity of seeing it; for the scientific journals in this country are so numerous, and, as far as my own science is concerned, so much the echo of each other, that instead of taking them all, I satisfy myself with a selection. The Journal of Mr. Brande, in consequence, I do not often see, as it is not even ordered for the College library. On perusing the review at the request of a friend, I saw that it required an answer, and accordingly gave such a one as was requisite for vindicating my character both as a chemist and a man. It would have been easy to have retorted with ten-fold interest upon the author of the review; for there are very few chemical writers indeed whose productions could be more easily exposed than the Andersonian Professor of Glasgow. But I abstained from retaliation, because I had no relish for cleansing such an Augean stable, and because I had no wish to degrade myself so far as to put myself on a level with Dr. Andrew Ure. The castigation which I gave was very moderate; though at the same time it was complete. Dr. Ure's answer I have never seen. I was aware that he could advance nothing that could better his cause, or require any additional reply from me; and I have laid it down as a rule never to read any ill-natured attack upon me, unless I have previously resolved to answer it.

I had heard of the present review from many quarters before it made its appearance; for the author, with his usual want of prudence, and buoyed up by a most uncommon stock of vanity and self-sufficiency, made no secret of his object; and mentioned every particular which he thought of consequence, or likely to hurt my feelings, and circulated it in Glasgow before his review appeared. And on its publication (after trying, it is reported, some of the newspapers in vain), he got it printed and circulated through Glasgow, in a weekly publication known by the name of M'Phan's Glasgow Mechanic's Magazine. I was aware, therefore, of the nature and object of the attack before it made its appearance; and wrote to my publisher in London to send me down a copy.

I had resolved, before I saw it, to make a few remarks on it; and I now sit down with that object in view. Even yet I shall refrain from inflicting the kind of castigation which the author so richly deserves, and which has more than once fallen to his share already, particularly from my friend Mr. R. Phillips, and the late Dr. Murray, of Edinburgh. At the same time I do not promise to be quite so abstemious as I was in my last reply. I observe that the tone of the writer is a good deal lowered, and that he speaks with rather more reserve of his own prodigious discoveries and improvements. Still, however, he indulges a

good deal in that gasconading manner for which he has been always notorious, and which constituted a standing joke against him in Glasgow when he was admitted into society. At present neither his gasconades nor his pretensions excite much attention in his native city.

My remarks on the present review will be short. I have no intention of noticing, far less of retorting, the abuse in which the author has indulged. I have too great a respect for my own character to engage in any such degrading employment. Indeed if I even felt the disposition to retaliate, I should be restrained by the simple consideration, that in the city where we both reside, all justification of myself is quite unnecessary. The bare name of the author of the review will be considered by every one who knows him as a sufficient refutation of every thing injurious which the review contains; and if I could suppose that the character and pretensions of this writer were as accurately appreciated at a distance as they are in Glasgow, nothing more would be necessary to divest the review of all its injurious effects than barely to make known the name of the author.

1. I shall begin my remarks by quoting the first paragraph of the review.

"The well-known author of this work regards the soul and body of chemistry to consist in a knowledge of the relative weights of the combining substances. This is to form a very narrow conception of the science. The true function of the chemical teacher is announced in the following verse of the Roman poet:—

> In nova fert animus mutatas dicere formas
> Corpora.

"It is the characteristic of chemical genius to reveal new elementary bodies, to form new compounds of the elements known before, to discover new qualities and relations both among simple and complex substances, and to arrange the manifold and marvellous phenomena of corpuscular action under a few general laws. The philosopher of ardent and inventive mind, content to know the general proportions, is unwilling to stop his career of discovery in order to learn the minute fractional quantities; nor will he suffer his whole faculties to flutter round the oscillations of a balance. Let none, however, hence imagine, that we desire to disparage quantitative research; we would only assign it a place of due subordination, below the qualitative, conversant with new powers and forms of matter."

Such is the elegant exordium of Dr. Andrew Ure—indicating, if the author affixed any meaning to his words, that accuracy in chemical researches is a poor low quality unworthy the attention of a man of genius. Now I wish to impress upon those gentlemen who are entering upon the career of experimental chemistry, that no opinion can be more erroneous, or likely to lead them

into so fatal mistakes. Without the minutest attention to precision, chemical researches are not only useless, but pernicious. The whole of chemistry, so far as it is entitled to the name of science, consists in the accurate measurement of quantities; and all the progress which it ever has made, or ever will make, must depend upon a rigorous and judicious use of the thermometer and the balance. Indeed all science consists in the determination of quantity; and the most eminent scientific men have not been discoverers of new substances; but have improved the sciences to which they devoted themselves by a careful comparison of the facts already known, deducing from them new general laws, which had not been formerly recognised. Was it not in this way that Newton acquired a rank in science to which no other person has yet reached? He added no new substance to the list of those already known. He made no augmentation in the number of planets, or of satellites, recognised before his time. But he did what was far more difficult. He showed that all the motions of the planetary bodies are the consequences of certain simple general laws. By pointing out these he has enabled his successors to determine the motions and the situations of all the planetary bodies for any period of time with the minutest accuracy, and thus to bring the science of astronomy in some measure to a state of perfection.

Lavoisier existed at a period when chemistry was cultivated in many parts of Europe with indefatigable zeal, and when new substances and new properties of old bodies were made known in considerable numbers almost every year. Lavoisier added no elementary substance to those already known. It can scarcely be said that he investigated the properties of a single chemical body; yet, assisted by the balance alone, he raised himself to a higher chemical rank than any of his contemporaries. How many new substances were discovered by Priestley, while Cavendish scarcely added one to those already known? Yet does not the latter rank much higher as a man of science than the former? To come to our own time, who has been a greater benefactor to chemistry, or who has merited a higher reputation, or will stand better in the opinion of posterity, than Mr. Dalton? Yet what new chemical element has he discovered? The discovery of new chemical bodies is undoubtedly a meritorious work, and the science is much indebted to those who have had the good fortune to add to their number. But in the present state of chemistry, it cannot be too forcibly inculcated, that he who adds to the precision of the facts already known is of more service to the science than he who adds to the number of elementary bodies. The science is still inundated by false facts, which are retailed and appropriated by compiler after compiler with the most careless indifference. Elementary books and dictionaries of chemistry are annually accumulating, and errors

are propagated till their origin ceases to be suspected, and till they come to be generally believed. As an example, I may mention that I happened sometime ago to turn up a System of Chemistry, of considerable celebrity, written by a chemist still alive, and possessed of reputation. The account of nitrate of silver caught my eye. I observed a minute statement respecting the water of crystallization of this salt. Knowing that the nitrate of silver is anhydrous, I shut the book, and have never since been able to prevail on myself to open it again.

He who will take the trouble to determine with minute accuracy the properties of all known bodies, will do more to advance the science of chemistry than he could accomplish by the discovery of a thousand new substances. The foundation of the whole fabric is an accurate knowledge of the atomic weights of bodies; because upon that knowledge the whole art of analysis, and consequently chemical experimenting, entirely depends. It is only since the atomic theory has been brought to a considerable degree of perfection that the art of analysis, and with it chemical science, has made a considerable approach to precision: and every successful attempt to add further to the perfection of this important theory is in reality of more value to the science than the discovery of a new simple substance; because every new law, or extension, or modification of an old law, is of more importance than any mere simple fact can be. I again, therefore, caution young chemists from being led astray by the tirade which I have just quoted. Indeed the author was obviously writing merely to answer a particular purpose; for every thing that he himself has attempted to do in chemistry has been confined to the ascertaining of weight and measure. Even in the present review, he piques himself upon his knowledge of the specific gravity, and the bulk of vapour at different temperatures; and such is the value which he sets upon some tables indicating the strength of nitric and muriatic acids, which it seems he has published; that he modestly insinuates that no one but himself was capable of constructing them—that the tables of these strengths which I myself have given must have been stolen from his; and that, despairing of being able to reach the degree of accuracy which he had obtained, I voluntarily deteriorated his precious labours in order to conceal the source from which I had acquired my information.

2. After his elegant exordium, which I have already laid before the reader, Dr. Andrew Ure, of the Andersonian Institution, Glasgow, proceeds to display his profound knowledge of the subject which he has undertaken to review. This he accomplishes by a process sufficiently simple, and very common among reviewers, who are ambitious of displaying their learning and extent of research without any expense of time and labour. He transfers my historical introduction into his review, and gives it

as his own, without the least acknowledgment, abusing at the same time the work to which he stands indebted for every thing that he happens to know of the subject.

3. It would serve no purpose to enter into any discussion respecting the reason why a volume of oxygen gas is equivalent to two atoms, while a volume of hydrogen gas is only equivalent to one, respecting which the reviewer has written so much, so absurdly, and so inconsistently. In a former review he informed the public, that this law, which he sneered at as ridiculous, was merely a blunder of mine, occasioned solely by my ignorance of the common rules of arithmetic. Even yet it is obvious that he does not understand the subject, though his confidence is greatly softened down, and his arrogant ignorance rather better concealed. He now tells us that it is the consequence of an arbitrary convention among chemists in general. Should he ever acquire chemical knowledge enough to take a general view of the combinations of gaseous bodies, he will find that instead of an arbitrary convention, it is founded upon an important law of nature. An arbitrary convention of chemists in general it certainly is not, since it never has been admitted by Berzelius and his pupils, a pretty numerous, and surely a highly respectable body of chemists; nor, so far as I know, by Sir Humphry Davy.

But much accurate information on the part of the reviewer, in his present situation, and with his present feelings, is an acquisition by no means probable.

> Qui dubitat, qui sæpe rogat, mea dicta tenebit
> Is, qui nil dubitat, nil capit inde boni,

is a maxim of old Lilly, which holds as truly in the present day as when that celebrated pedagogue committed it to paper.

4. Neither is it worth while to examine the arguments which the reviewer brings forward to show that hydrogen constitutes a better atomic unit than oxygen. It is not of much consequence what unit be adopted, provided chemists agree about the proportions. I make oxygen unity because I think proportions are more intelligible the smaller the numbers are by which they are represented; but in the tables which constitute an appendix to my late work, I have given both scales, to accommodate those who prefer the hydrogen scale to the oxygen one. The arguments of the reviewer could not fail to be amusing, knowing, as I did, that he has been in the habit himself of making use of the oxygen scale. Nothing, therefore, but the spirit of contradiction, and the ambition of saying what he considered as smart things, could have induced him in his review to embrace the side which he has taken. When he says that hydrogen enters into more numerous and more interesting combinations than oxygen, the statement is in direct variance with our present

chemical knowledge. Abundance of new combinations of elementary bodies indeed still remain unknown; but hydrogen, from its peculiar qualities, is less likely to admit of our uniting it to elementary bodies in general than oxygen, or any other chemical body whatever. The only observation of the reviewer upon this subject which has any force is borrowed from Dr. Prout, and borrowed too as usual without acknowledgment.

5. By far the longest and most elaborate part of the review, and what was intended to constitute the grand display of the knowledge and sagacity of the reviewer, is the attack upon the third chapter of my work. It occupies eight pages, and was viewed by the author with much self-complacency. What betrays the cloven foot of the reviewer is the qualification with which he takes care to guard every observation. He admits the truth of all my conclusions, satisfying himself with denying the validity of all the premises. Now as the opinions which I have advanced in my third chapter, though at present pretty generally admitted, were, when I first gave them to the public in the *Annals of Philosophy*, almost universally opposed, and by none with more violence than by the author of the review; it is certainly a most extraordinary accident, to say the least of it, that should have led me to right conclusions from inaccurate experiments and delusive reasoning.

For a full refutation of every thing contained in this redoubtable part of the review, I refer the reader to my answer to Mr. Rainy, inserted in the *Annals of Philosophy* for November last, and to my account of the analysis of sulphate of zinc printed in the same number of that journal. It will be seen there that I committed no mistake in my calculation, and that the experiments were as near the truth as the most minute attention to accuracy could enable me to go. The mistakes belong to the author of the review, who left out of view a most important part of the data, the boiling point of the acid which I employed.

It was fortunate for the reviewer that this answer of mine did not make its appearance till after the publication of the review. It might have obliged him to have cancelled the most imposing part of his essay, which would have obliterated a whole budget of witticisms and repartees upon which I had been told that he valued himself not a little. He even boasted, it is said, to some of the few individuals in Glasgow who still condescend to associate with him, that he would demolish all my five years' labours in five minutes.

The witty remarks upon the capacity of the flask which I employed for dissolving the zinc in my experiments to determine the specific gravity of hydrogen gas, are upon the whole the most passable in the review. I have some thoughts, therefore, of sending the flask to the managers of the Andersonian Institution, that they may place it in their museum with a suitable

inscription, as a memorial of the great prowess, the unimpeachable candour, and the gentlemanly feelings of their worthy professor. It is true that it met with a small accident about six weeks ago, while I was showing its capacity to a gentleman who had called on purpose to make inquiry on the subject, after the appearance of this most formidable review. The accident is merely a trifling crack, not far from the mouth. In other respects the flask is still entire, and is very much at the service of the managers of the Andersonian Institution. Perhaps indeed the crack may rather contribute to add to the value of the flask, as it will render it still more emblematical of the illustrious reviewer whose mighty deeds it is intended to commemorate. From the regard which the managers have lately manifested for their worthy professor, I am led to believe that such a testimony of his achievements would be most acceptable to them.

A regard for veracity obliges me, though with regret, to contradict one of the most brilliant statements in this part of the review. The author informs us that he repeated my experiment; but that he took the precaution at the same time to insert a thermometer into the flask containing the dilute sulphuric acid and zinc, and that it stood 11° higher than the temperature of the water in which the flask was plunged. Now in my experiments 24 hours elapsed before the zinc was fully dissolved. Had the flask continued the whole of that time 11° above the water in which it was plunged, the quantity of heat given out during the solution of 130 grains of zinc must have been at least sufficient to have heated six cubic inches of very dilute sulphuric acid 1500 degrees! Had the reviewer paid any attention to the account which I gave of my experiments, he would have seen that I observed the temperature during the solution with the most careful attention. The utmost rise when the experiments were made in the way that I have described in my answer to Mr. Rainy, did not exceed $1\frac{1}{2}$°; and during a very considerable portion of the time, there was no rise of temperature whatever.

6. The reviewer roundly asserts that my table of the atomic strength of muriatic acid was copied from one which it seems he published in the Journal of Science for January, 1822. I was a little startled at learning the existence of this table, of which I was not aware till I saw it noticed in the review. I am disposed to suspect that a theft has indeed been committed. My table was exhibited to the students of chemistry in Glasgow College (known to be a pretty numerous and annually increasing body) for the first time during the winter session 1820-21. Many copies of it were taken and dispersed. The review carries internal evidence that the worthy Professor of the Andersonian Institution is not a little upon the alert to know what is going on in the College laboratory. Indeed I know from positive information that he has more than once tampered with my

students. I strongly suspect that a copy of my table found its way to the Andersonian Institution, and that it was thought too good a thing not to be appropriated. Whether this conjecture be well or ill founded is of little consequence. The priority in point of date is indisputably mine. Indeed had Dr. Ure even published his table before mine was drawn up, I would have still submitted to the labour of the requisite experiments; because I had formed the resolution of subjecting every chemical fact connected with my subject to a new and rigid examination, whenever it was in my power to do so. Dr. Ure's table published in the *Annals of Philosophy* for 1817 is everywhere erroneous to the amount of about 33 per cent. The author was then a supporter of the old doctrine respecting chlorine and muriatic acid; and after having demonstrated the truth of his opinion by most irrefragable experiments, which the late Dr. Murray declared had been stolen from him, he turned to the right about, and embraced a theory which he had just before demonstrated to be false!

My table of the strength of nitric acid was drawn up during the winter 1821-22. The greater number of the specific gravities on which it is founded were taken by Mr. Colquhoun, of Glasgow, who was at that time my assistant, with a degree of care and attention which could not easily be surpassed. I have, therefore, great confidence in the accuracy of the table. Dr. Ure's table I have never yet seen. It may be very accurate and very useful. But had I been in possession of a copy of it, I should still have considered it as requisite to draw up my own.

It was this determination to take nothing on trust that was the cause of the great length of time which my experiments occupied. They were all made with the most scrupulous attention to precision, and were all as accurate as the means in my power enabled me to render them. When the substances were common, the experiments were so frequently repeated, and so varied, that I have the utmost confidence in the results. When the substances were scarce, and when I was restricted to small quantities, the risk of error was much greater. This was the case with palladium, of which I possessed only a few grains, and respecting the atomic weight of which I could not have acquired any satisfactory knowledge, had not the previous experiments of Berzelius enabled me to calculate in some measure beforehand. By the liberality of Dr. Wollaston, I have been lately supplied with some ounces of palladium. I have been enabled in consequence to repeat and vary my experiments, and to extend them to several other salts of that metal; and have had the satisfaction to find that the atomic weights of palladium and its oxide, given in my late work, are fully confirmed by these new investigations.

I may notice here the Doctor's modest averment, that the first analysis of oxalate of ammonia was made by himself. The

constituents of this salt, exactly as I have stated them, were communicated to me by Dr. Prout before I left London in 1817. Berzelius had analyzed it at least as early; for he gives its constituents in the tables attached to the third volume of his Larbok i Kemien, published in 1818, which was translated into French, and published by himself in 1819, under the title of *Essai sur la Théorie des Proportions Chimiques*, a book which Dr. Ure has no doubt seen. It requires no little command of face to claim coolly as a discovery made by himself in 1822 an analysis published by Berzelius in 1818. I never dreamed that I was claiming as my own exclusively all the analyses which I have given in my late work. It is indeed true that two-thirds of them at least are new, and that all of them were repeated in my laboratory either by myself or my pupils, under my superintendence and direction. This was the object, and the sole object in view. Historical details and claims of priority I left for my System of Chemistry, in the various editions of which they demand and obtain a place. I was not aware that Dr. Ure had favoured the public with an analysis of this salt, and had I known it, I should have seen no propriety in noticing his posthumous labours.

7. The assertion that my experiments on the atomic weight of lime, and the specific gravity of carbonic acid gas, are fictitious, merits no answer, and shall receive none. I had occasion, during my long and laborious researches on these important subjects, to examine the validity of some previous experiments of the worthy author of the review, by which he assured us that the carbonates could be analyzed with the utmost expedition and accuracy. I made no allusion to these experiments in my late work, because I take no pleasure in exposing the mistakes of others. Even here I should have been silent on the subject, had the Doctor left me any alternative. I have not a doubt that fictitious statements upon this subject have been palmed upon the public. It was very natural for the author of the review, conscious of the existence of his own previous statements, to which he durst not venture to allude, to turn up his nose at my researches; which he good-naturedly allows to be accurate as to the conclusions, but impossible as to the premises. There is a proverbial allusion to this very common mode of conduct too indelicate to be quoted here; but well worthy of the Doctor's serious consideration, that he may know by it how to regulate his conduct hereafter.

8. I beg leave to present my reader with another quotation from the review. "Now we affirm that this experiment, from which he deduces the atomic weight of chromium, was never made, for the result is impossible. *Ammonia does not precipitate oxide of chromium from the above green solution in tartaric acid.*"

The method here alluded to by the reviewer, and pro-

nounced impossible, ex cathedra, is as follows: Chromate of potash is dissolved in water, and tartaric acid is added to the hot solution till it acquires a green colour. Ammonia added to this solution throws down the protoxide of chromium in the state of a hydrate. It seems the worthy Doctor had been trying his hand at this experiment; but had been unsuccessful: and such is his high opinion of his own prowess that he deems every thing impossible which he cannot himself perform. But this is, perhaps, rather narrowing the scale of possibility too much; for I am humbly of opinion that many things are not only possible, but easy, which the Doctor cannot accomplish. Thus I know that my friend Mr. Luke Howard can manufacture most beautiful crystals of *tartaric acid*, and that my friend Mr. Ramsay, of Glasgow, can manufacture excellent crystals of *bichromate* of potash; yet if we were to apply the Doctor's test of possibility, we should conclude that neither the acid nor the salt could be manufactured at all. I shall be good-natured enough to explain the cause of the Doctor's failure, and thus render the preparation of protoxide of chromium by means of tartaric acid and ammonia, a possible process, even when conducted by the worthy Professor of the Andersonian Institution, Glasgow.

Tartaric acid has the property of combining at once with potash and most other salifiable bases. If you digest bitartrate of potash and protoxide of chromium mixed in the atomic proportions, and with the requisite quantity of water, a complete solution will be obtained, having a deep bluish green colour, and almost opaque. When this solution is evaporated to dryness, a greenish black powder remains, which is a compound of two atoms tartaric acid, one atom potash, one atom protoxide of chromium, with a certain quantity of water of crystallization. This salt is readily soluble in water, and like the other compound tartrates is not precipitated by ammonia, nor by the fixed alkalies or their carbonates. This property of the compound tartrates has been long known to chemists; but it is particularly insisted on in a note to a very valuable paper of M. H. Rose, printed in the Memoirs of the Swedish Academy for 1820.

Dr. Ure had dissolved chromate of potash in water, and had added not merely the quantity of tartaric acid necessary to reduce the chromic acid to the state of protoxide; but enough to saturate the potash and protoxide of chromium in the solution. He had thus formed the compound salt which I call *potash-tartrate of chromium*. Now from the solution of this salt when it has been once formed, the protoxide of chromium cannot be precipitated by ammonia; but even when this blunder has been committed, the remedy is easy. Evaporate the liquid to dryness, ignite the dry mass to destroy the tartaric acid, digest the black residue in water to dissolve out the potash. A black powder will remain which is a mixture of charcoal and

carbonate of chromium. Muriatic acid will readily dissolve the protoxide of chromium. Filter the solution to get rid of the charcoal, and evaporate it to dryness to get rid of the excess of acid. If the muriate of chromium thus obtained be dissolved in water and mixed with ammonia, protoxide of chromium will be precipitated in abundance. If Dr. Ure will follow the simple directions here given, I shall pronounce him a bungler indeed if he cannot obtain protoxide of chromium in any quantity whatever.

9. If the reader will indulge me in one quotation more from this most delectable morsel of criticism, I will not trouble him again. But I am unwilling to withhold any thing which is likely to display the uncommon candour, profound knowledge, and gentlemanly feelings, of the worthy Andersonian Professor of Glasgow. The passage I allude to is as follows: "His number for tartrate of potash is unquestionably wrong; and, indeed, though it were right, his conclusion would be erroneous; for the *mother-water* (as he elegantly terms the limpid supernatant liquid) contains, under his proportions, both tartaric acid and oxide of lead. Let it be tested with sulphate of soda, and it will become cloudy; with sulphuretted hydrogen, and it will become very black; or with nitrate of lead, and tartrate of lead will fall. Thus the principle of Richter, of whose application our Doctor is so vain, becomes under his management quite deceptious." (Review, p. 138.)

As I consider the method which I took to determine the composition of tartrate of potash to merit the attention of practical chemists, and as I made no allusion to the principles on which this method is founded in my late work, I shall take the present opportunity to point them out. There are some acids which cannot be precipitated completely from saline solutions by any reagent in our possession. Tartaric acid is in this predicament. Tartrate of lime is the most insoluble of all the tartrates, but the salts of lime do not precipitate this acid, unless we evaporate the mixture to dryness; but tartaric acid is so liable to be altered by heat, that this method is not quite safe. Nitrate of lead precipitates tartaric acid immediately; but tartrate of lead is not insoluble. Of course when nitrate of lead and tartrate of potash are mixed in the atomic proportions after the precipitate has subsided, a little tartrate of lead will remain in solution. I take care to concentrate this liquid so much that the tartrate of lead which it retains can amount only to a small fraction of a grain. I then test it with nitrate of lead and tartrate of potash—neither of which salts are capable of acting upon the tartrate of lead present; but if either lead or tartaric acid exist not in combination with each other, these reagents will indicate its presence. By this method we have it in our power to determine the quantity of tartaric acid in a salt suffi-

ciently near to be sure of the atomic composition of the salt under examination. The application of sulphate of soda and sulphuretted hydrogen is out of the question, because these reagents would act upon the tartrate of lead in solution. Were nitrate of lead to act indeed, as the author erroneously states, the inference would be that the tartaric acid in the mixture was in excess.

In the analysis of tartrate of potash, I did not satisfy myself with determining the quantity of tartaric acid in a given weight of the salt, but ascertained likewise the amount of the potash. My method was to ignite 16·5 grains of the crystals in a platinum crucible, and to add to the solution nine grains of oxalic acid in crystals. The potash was exactly saturated by this weight of acid; for the solution, after being heated to drive off the carbonic acid, did not alter the colour of litmus or cudbear paper.

10. I do not know whether the remarks made by Dr. Ure upon the non-precipitation of lead by sulphate of soda be in jest or earnest. He would scarcely have hazarded them, I think, unless he had reckoned rather too much upon my forbearance, and on the ignorance of the reader; for so far as I know it was I myself who first pointed out this remarkable fact in a paper printed a good many years ago in the *Annals of Philosophy*. To advance a fact which I first pointed out as a proof of my ignorance of the value of the reagents which I employed, is a degree of barefaced impudence to which every chemist in Great Britain would have been unequal, except the Andersonian Professor of Glasgow.

11. There is one observation more of the reviewer to which it will be requisite to attend before I conclude. He says that the best way of obtaining pure salts is to take them as they are prepared by manufacturing chemists. Were this recommendation to be adopted implicitly by young chemists, it would be productive of most injurious mistakes. The competition in this country among manufacturers is at present so great that they are under the necessity of selling at too low prices to be able to produce their salts in a state of complete purity. Caustic potash is always contaminated with lead. Muriate of barytes as it comes from the makers is usually in the same predicament, and unless it be purified will be unfit for use as a reagent. Nitrate of silver always contains gold, often copper, and sometimes potash.* Carbonate of soda always contains sulphuric acid. Bicarbonate of potash is never free from lime. I have found sulphate of lime in the magnesia of commerce; carbonate of zinc is a constant ingredient in acetate of zinc. Sulphate of zinc is never, and sulphate of copper seldom free from iron. Alum also is frequently contaminated with the same metal. The muriatic acid of commerce is never free from sulphuric acid,

* Last summer I was favoured with a specimen of this salt prepared for sale in Dublin, which I found perfectly pure.

and this is the reason why its specific gravity is so high as 1·22. When I spoke of muriatic acid in my work, I meant pure acid, and I have never been able, even in winter, to obtain it higher than 1·212.

I have now noticed every thing in the review that seems to me entitled to attention, or likely to mislead the young chemist. I shall conclude by observing that in my late work I have given 747 sets of experiments, most of them analyses, a considerable proportion of which are new, and all were performed in the College laboratory of Glasgow, either by myself or my pupils under my immediate directions and superintendence. A very considerable proportion of these analyses were repeated many times before I felt satisfied with the result. This labour, the greatest hitherto undertaken by any individual chemist, occupied the whole of my attention for five years, during the greatest part of which time I was as assiduously employed as any journeyman in Glasgow. Now let us sum up the result of Dr. Ure's animadversions. He affirms (without assigning any reason for his statement) that three of my analyses are erroneous, that three are fictitious, and that I have stolen three from his own immaculate publications. Now were we to grant the truth of this impudent and most profligate statement, it would still follow, even by his own showing, that 735 of my analyses are perfectly accurate.

ARTICLE II.

On the Origin of Ergot. By General Martin Field.*

As to the origin and nature of ergot, various opinions and theories have been adopted; but the three following have appeared the most plausible, and have been the most strenuously supported. First: among the French, Tissot and others affirm, "that ergot, or spurred rye, is such as suffers an irregular vegetation in the middle substance between the grain and the leaf, producing an excrescence;" and that this morbid change is produced by the extremes of humidity and heat of the season.

Second: In England it has been the opinion of some, "that ergot is an excrescence, caused by the sting and deposition of the eggs of an insect."

And third: Others affirm, "that it is a parasitic *Fungus*, like the different sorts of blight, smut, &c."

I shall not attempt to support or oppose either of the opinions above-mentioned; but shall relate such facts as have fallen under my view, without regard to any theory upon the subject.

The field of rye in which I made my observations was within fifty yards of my house, which afforded me an opportunity of

* American Journal of Science.

examining it daily for many weeks. It was of that species, which, in this part of the country, is usually denominated the *Norway or White Rye,* and which has ever been observed to be far more productive of ergot than the English spring rye, or that which is said to be a native of the island of Candia. But it is not recollected, that during any former season, the ergot has been found so abundant, in this vicinity, as during the last.

The Norway rye is in blossom about as early in the season as the English spring rye; but is two weeks later at harvest. From this circumstance, one reason may be assigned, why the former is so much more productive of ergot, than the latter. The longer the grain continues in the *pulpy* or *milky* state, the more favourable is the opportunity presented for the operation of the cause which produces the ergot. That such is the fact, experience clearly demonstrates.

The field of rye which I very frequently examined, was in full blossom about the 30th of June; but I discovered no appearances of ergot till the 22d of July. From that time, until the 12th of August, when the rye was harvested, it might be found of various dimensions. Upon minute examination, I discovered that every grain of ergot, as it emerged from the glume, had attached to its apex the shrivelled rind of a grain of rye, which had the appearance of once being in a healthy state. This led me to conjecture that a diseased state of the rye was the primary cause of the ergot. To ascertain the fact, I repaired to the rye-field, where I discovered groups of flies collected upon the heads of rye, apparently in the pursuit of something within the glume. On opening the valve of the glume, where the flies were thus collected, I found the saccharine juice of the grains of rye was oozing out, and would soon produce small drops. I was then convinced that it was this saccharine fluid which was so inviting to the multitude of flies that collected upon those heads of rye, which contained any diseased grains. Having collected a number of grains, of full-grown size, and exhibiting appearances similar to those above described, I placed the same under a microscope, by which I could clearly discover a small orifice in each, near the end opposite to that to which the *thread of nutrition* had been attached. I could also discover the juice of the grain was still discharging from the orifice.

On the morning of the first of August, by observing the groups of flies, I found two heads of rye near each other, and each of which contained a grain of *punctured* or diseased rye. The culms I tied to a stake, drove between them, the better to enable me again to find them, and to observe their future appearances. At that time the *punctured* grains exhibited no symptoms of decay, otherwise than a small discharge of fluid. During the first day, the flies were busily employed in extracting their delicious beverage from the orifice of each grain, and when it did not flow in sufficient quantity for their supply, they would

probe it anew. On the 2d of August both grains appeared to be in a state of fermentation, and rapidly tending to decay. On the 3d, being forty-eight hours from the time when I commenced my observations, each grain had become a rotten and shapeless mass, and exhibited very little appearance of healthy rye. Then on carefully opening the valves of the glume, I discovered in each a small black globule, the size of which was rather larger than a pin's head. These were situate at the points of the *peduncles* of the diseased grains, which afterwards proved to be ergot. During the first four days after the ergot was discovered, they grew in length very near two lines in each 24 hours, displacing the remains of the diseased rye from the glumes which they had occupied. On the 12th of August, the ergot had attained its full growth. The dimensions of one grain of ergot were 12 lines in length, and three lines in diameter. The other grain measured a little less.

On the 3d of August, being convinced that the primary cause of ergot was the puncture of the healthy grain by the fly, it occurred to me that perhaps it might be produced by such means as I possessed. To ascertain this fact, with the point of a fine needle I *punctured* four grains of rye, in the same head, it then being in a green pulpy state, and of full grown size. A discharge of the juice of the grains was soon discovered from the orifice of each. The flies collected as in those cases before mentioned. The result was, that on the fourth day after the operation was performed, ergot appeared in the glume, occupying the places of two of the punctured grains. The other two grains exhibited no symptoms of decay, but continued in a healthy state. From appearances, I am led to believe that in warm dry weather many grains of rye are punctured, which are not materially injured thereby. The orifice closes before a sufficient quantity of juice has escaped to produce fermentation and decay. This may, therefore, be assigned as one reason why cloudy and wet seasons are so much more productive of ergot than those which are fair and dry.

Under a good microscope, I occasionally examined the ergot, and also the grains of rye, in every stage of decay, but was never able to discover in either the eggs or *larvæ* of any insect. I, therefore, conclude that the puncture of the fly is for the purpose of extracting its food from the rye, and not for the deposition of its eggs.

The fly is of the hairy or bristly species of *Musca*, and also a species of the "blow fly." It deposits its eggs upon animal flesh, either fresh or putrid. Its wings are transparent, abdomen dark green, larger than the common house fly; in this climate, in the months of July, August, and September, the most numerous species of the fly, and very annoying to horses, oxen, and some other animals.

The above statement contains all the material facts which

have fallen under my view, in relation to the cause of ergot; and how far they go to support or oppose either of the theories heretofore adopted upon the subject, I submit to the decision of others.

In the conclusion of this article, perhaps it may not be improper to state some facts in relation to the effect which the ergot produces upon the health of the plant on which it grows. I was never able to discover that the culm of rye was in the least affected by the ergot; but I have observed that invariably where there were to the number of eight or ten grains of ergot, no healthy or sound rye could be found in the same head. In such cases it appears that all the nourishment which the culm affords is exhausted by the ergot, and the rye suffers a severe blight.

The size of the ergot is usually in proportion to the number of grains in the same head. For when we find but one grain in a rye-head, it is generally from 10 to 14 lines in length, and two or three in diameter; but where there are from 25 to 30 grains, which is not unfrequent, their dimensions are proportionably less, being often not greater than sound rye.

Article III.

Analysis of the Ore of Iridium. By T. Thomson, MD. FRS.

In a paper inserted by Dr. Wollaston in the Philosophical Transactions for 1805, he gives an account of an ore of iridium which is intermixed with the grains of crude platinum, and which remains unacted upon after the platinum has been dissolved in nitro-muriatic acid. He picked out a number of these grains, and gave an account of some of their most remarkable properties. They have a foliated texture, a peculiar lustre, are brittle, and have a specific gravity of 19·5; thus constituting the heaviest body hitherto discovered in nature. Dr. Wollaston informs us that by analysis he could detect in it nothing but iridium and osmium. He selected a portion of this ore, and gave it to the late Mr. Smithson Tennant, requesting him to make an exact analysis of it.

Unfortunately this amiable and highly ingenious man was cut off by a most melancholy accident before he had found leisure to execute the projected analysis. I had received a quantity of the same ore from Dr. Wollaston many years ago; but as I considered the investigation of the metals in crude platinum as in some measure forbidden ground, I was unwilling to obtrude upon it while there was any reason to expect that Dr. Wollaston himself might be prevailed upon to undertake a task which he was so much better qualified for than any other person. But as

twenty years have elapsed without any thing having been added to our knowledge of this ore, I was induced to attempt the analysis of it on the supposition that it was likely to throw some light upon the atomic weight of osmium, which I had tried in vain to determine by other methods.

After some preliminary experiments, I found the most successful method of proceeding was to heat the ore with a mixture of caustic potash and nitre. A platinum crucible could not be employed, because at the heat which I found it necessary to use, the crucible was sensibly corroded. I tried green glass vessels with no better success; and at last made choice of a silver crucible, which, when the quantity of saltpetre was not too great, was not sensibly acted on at a red heat.

Ten grains of very pure plates of ore of iridium were heated in a silver crucible over a lamp in a mixture consisting of about 50 grains of potash and 20 of saltpetre, and kept for about half an hour in a heat approaching to redness, and ultimately quite red. The potash and nitre soon fused, and became quite black and opaque. When all action seemed at an end, the process was stopped, water was poured into the crucible, and the whole potash and nitre washed out. An opaque liquid was thus obtained, which, when left for 24 hours, deposited a number of black flocks. The residual liquid had an olive-green colour. When heated, it deposited some more black flocks, and became colourless. It had a strong smell of osmium.

The residual ore of iridium was black on the surface; but being digested for 24 hours in muriatic acid, it assumed the metallic lustre as at first. The muriatic acid liquid remained transparent; but had acquired a reddish brown colour. The ore of iridium by these two processes was reduced to 7·35 grs.

Being again heated with potash and nitre, and afterwards digested in muriatic acid, it was reduced to 6·37 grains.

After being subjected a third time to this treatment, it was reduced to 4·45 grains.

A fourth heating with potash and digestion in muriatic acid reduced it to 2·78 grains.

A fifth to 2·3 grains.

A sixth to 1·5 grain.

A seventh to 1·18 grain.

This small residue had the metallic lustre, and exactly the characters of the original one. I, therefore, stopped the process here. If we subtract the 1·18 grain of residue from the 10 grains taken, there will remain 8·82 grains for the quantity actually subjected to analysis.

All the potash solutions were collected together and boiled in a flask till they became limpid and colourless. Osmium was given out, and a black flocky precipitate fell, which, being collected and dried on the filter, weighed 5·4 grains.

This black deposit was light and bulky. When exposed to the heat of a spirit-lamp in a small porcelain crucible, it took fire, and burnt for an instant, owing, I suspect, to some small vegetable matter from the filter being mixed with it. It retained its black colour after having been exposed to a strong red heat, but the weight was reduced to 4·765 grains. When this powder was digested in nitro-muriatic acid, a red solution was obtained, which became colourless by the addition of gallic acid, and could not be precipitated by the action of any reagent tried. Hence it was iridium. A complete solution of the black powder could not be effected by nitro-muriatic acid, showing that the iridium was in the metallic state.

The muriatic acid solutions being collected together and evaporated to dryness, left a black matter weighing 3·3 grains; but reduced by exposure to a strong red heat to 1·89 grain. This portion had been united to the 4·765 grains of iridium obtained from the potash, and the whole was digested for a week on the sand-bath in muriatic acid. The red-coloured liquid became colourless when ammonia was added to it in excess; but a slight flocky precipitate fell which had a brown colour, and weighed, after being dried in a red heat, 0·33 grain. This matter dissolved in muriatic acid. The solution was reddish yellow, and struck a deep blue with prussiate of potash. Hence the flocks were peroxide of iron, equivalent to 0·23 grain of metallic iron. The colourless solution thus freed from iron being evaporated to dryness, and redissolved, assumed its original red colour.

Thus from 8·82 grains of ore of iridium were obtained,

Iridium....................	6·43	or 7·5
Iron........................	0·23	0·277
Loss (obviously osmium).....	2·16	2·51
	8·82	

I have shown in a late work that the atomic weight of iridium is 3·75. We see from the preceding analysis, which I had the patience to perform twice, notwithstanding the tediousness of the manipulations, that the ore of iridium (abstracting the iron, which is probably only accidentally present) is composed of three parts iridium and one part osmium. If we consider it as a compound of two atoms iridium and one atom osmium, the atom of osmium will be 2·5. If it be a compound of one atom of each constituent, the atom of osmium will be 1·25, or the same as the atom of fluoric acid. We have not data at present to determine which of these suppositions is the true one. I shall, therefore, make choice of the first number provisionally till we have it in our power to determine the point with more accuracy.

Article IV.

On Naval Architecture. By George Harvey, Esq. FRS. L. & E.

(To the Editors of the *Annals of Philosophy.*)

Gentlemen, *Plymouth, Nov. 5, 1825.*

No better method, perhaps, can be adopted for benefitting naval architecture in its present state, than by making it occasionally the subject of friendly discussion in our philosophical journals; of freely stating what are its wants and defects; what points require elucidation; which of its elementary principles are involved in obscurity; and what ought to be the means employed to carry it to that pitch of perfection its ardent and eager cultivators desire.

Without meaning to cast the slightest reflection on those who have preceded us in the cultivation of this interesting and important art, it ought freely to be admitted that much remains to be done for its improvement; and that as every nation almost is aiming more or less at a maritime character, and that we have around us so many countries jealous of our national superiority, of our wealth and commercial prosperity, and of all those advantages which so pre-eminently distinguish Britain above every other nation, it behoves us as individuals, and collectively as a nation, to spare no pains in giving to our naval and commercial marine every degree of perfection of which they are susceptible.

The plan suggested by Mr. Major in the *Annals of Philosophy* for November, is one well calculated to awaken attention; and though the undertaking may at first view appear too wide and extended to be attempted in all the generality proposed, yet if the best ships of each class were selected, and due consideration used, to free it in its execution from those difficulties and embarrassments which sometimes so unexpectedly arise to impede the experimenter, the most important consequences to naval architecture might be reasonably anticipated from it. A digest of the British navy, which should embrace *only* the elements proposed by Mr. Major, *properly methodized and arranged,* would form a body of knowledge of so important and peculiar a kind, that it would be difficult to estimate its value. To me, I am free to confess, the idea is new; nor in my estimation is novelty the most important of its characteristics. It is, in truth, carrying at once into the very heart of ship-building that spirit of genuine induction which, in so many other branches of knowledge, has produced such mighty consequences; nor do I know of any one thing so likely at the present moment to increase our stock of information respecting this useful art as the scheme now proposed for consideration.

We have much to learn respecting ship-building. We ought certainly to know every thing capable of being known respecting it; and much might be gained by experiments founded on a philosophical basis. It would be of great importance, for example, to have precise and definite notions respecting the plane of floatation; how that plane is connected with the midship section; and how the two are related to the sailing qualities of the vessel of which they form a part. In like manner, it would be advantageous to have correct and accurate ideas respecting the positions of the centres of gravity of the entire vessel and of its displacement; and to know how these points are related to each other, in a given number of approved experiments. Is there indeed a single element connected with naval architecture which in the present state of our knowledge it is not desirable to improve?

Let us inquire for a moment how we obtain information in other cases; how the philosopher works in his difficult investigations; and what are the instruments and means employed by him when tracing the hidden mysteries of nature. Are they not *experiment,—observation,*—a careful watching after resemblances and relations of every kind? Does he not analyze every principle, separate every part, and in the end collect into general and connected laws the individual results which his sagacity has discovered? Just so ought it to be in the pursuit of naval architecture; for there are about that subject, elements of a very peculiar kind whose individual properties and collective laws it is of the highest importance to determine. Much may indeed be said about theory; but pure theory has yet done little for ship-building. What we want is a theory *founded on the basis of experiment and observation;* and as I have remarked on another occasion, without it "all is darkness and uncertainty."* The first mathematician in Europe may speculate for ever on the forms of floating bodies; he may dazzle his imagination with his ideal creations; he may multiply his analytical combinations, and pile his highest orders of integrals on each other; and yet, when called upon to make his practical applications, his formulæ almost lose their identity, and all his golden speculations vanish. But place in the hands of such a man a well-digested body of experimental results; show him how, in numerous instances, one property of a vessel has been invariably connected with another: give to him those *constants* which are to link together the disjointed elements of his problem; furnish him with *experimental data* on which he can depend, and from which he can with confidence draw such results as his growing investigations require; and we shall find in the end a striking contrast to his former results. The data supplied to him will

* *Annals of Philosophy,* vol. viii. p. 445, New Series.

have disclosed relations never before anticipated, and conclusions never before imagined. Thus would naval architecture be benefitted; and an art, which, it is not too much to say, is of the very first importance for the British nation to cultivate and encourage, would be freed from the trammels of uncertain and antiquated rules, and placed on a basis better suited to its dignity and value.

I have one word, however, to say on resistances—a subject which I contend it is of very great importance for our naval constructors to be acquainted with. It is no argument because the labours of the French Academicians, and of the Society for the Promotion of Naval Architecture, have not produced all that practical benefit which might have been anticipated from their high talents, and the unwearied assiduity with which the subject was prosecuted by them, that therefore no better information could be derived from a course of experiments on some other plan. Rather should such difficulties and obstacles operate as a stimulus to new exertions; to inquiries *why* so much apparently well-directed labour should have produced so little that is of practical value, and not in hopeless despair abandon an inquiry because our predecessors achieved not all we desire.

Finally, it is much to be desired that we should be furnished, before the inquiry is undertaken, with a table of the principal *elements* which such an investigation as that under consideration ought to embrace, arranged in the order best adapted to the relations they bear to each other. A methodical statement of our actual wants in the first place would be one step towards their attainment; and no better method, perhaps, can for the present be devised, than arranging the objects desired in *tabular* and *systematic forms.* Nor should those forms embrace merely the ultimate results of the elements required, but the necessary and essential steps on which the respective elements depend. This plan of tabulating the steps and results of our inquiries is one of very great importance in the present state of the sciences, since it exhibits at once what are our actual possessions, and what further aid we require to lead them onwards to perfection. The advantages of this method of conducting investigations are now beginning to be felt, and, as knowledge and the useful arts extend, it will become more and more necessary.

Article V.

Observations on some Mineral Species. Extracted from a Letter of M. Berzelius to M. Alexander Brogniart.*

Stockholm, March 15, 1825.

My labours during the last winter have been sometimes devoted to mineralogical subjects. I think I have already informed you that the mineral so similar to zircon, which M. Tank showed us at Christiania, and in which I found phosphoric acid by means of the blowpipe, is a *phosphate of yttria*. We also observed another mineral found in the sienite of Fredericwern, black, very brilliant, and formed of small rectangular prisms. I have analyzed it, and found in it oxide of titanium, zirconia, yttria, lime, the protoxides of iron, manganese, and cerium, and traces of the oxide of tin, potash, silica, and magnesia. I have named it *Polymignite*, from the multiplicity of its elements. The *Levyne* sent me by Dr. Brewster is absolutely nothing more than chabasie, in which a portion of the lime is replaced by soda. The *mesola* which I analyzed, and named somewhere in Brewster's Journal, is in the same predicament; it is merely a chabasie rich in soda.

I have analyzed the last portion that I had left of the mineral containing *Thorina*. I found in it phosphoric acid, united to oxide of cerium and yttria; and I have discovered that this pretended earth is nothing else but a sub-phosphate of yttria. One element, therefore, must be erased from the catalogue.

I have examined two arseniates of iron, one from Villa-Ricca, in Brasil, the other from Wurfelerz. I find the formula of the first = $\ddot{Fe}\,\overset{\cdot\cdot\cdot\cdot\cdot}{As} + 2\,\dddot{Fe}\,\overset{\cdot\cdot\cdot\cdot\cdot}{As} + 12\,Aq$. That is to say, it is a neutral arseniate of the protoxide, in which two-thirds of the latter are converted into deutoxide. (*C'est-à-dire, que c'est l'arséniate neutre du protoxide, dans lequel deux tiers de celui-ci sont convertis en deutoxide.*) The formula of the *wurfelerz* is $\ddot{Fe}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}^2 + 2\,\dddot{Fe}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}^2 + 36\,Aq$. It is the common sub-arseniate of the protoxide, in which two-thirds of the protoxide are converted into deutoxide.†

The scorodite of Saxony is not identical with either of these two arseniates. We, therefore, know three different native arseniates of iron. This analytical labour has led me to revise

* From the Annales des Sciences Naturelles.

† Contrary to our usual custom, we have copied the symbols of the original text, not from any late conviction of their utility, but lest, by endeavouring to translate them into common language, we might fall into error. What a sub-arseniate of a *protoxide*, two-thirds of which consist of *deutoxide*, may be, we are somewhat puzzled to comprehend.—*Ed.*

all the analyses of the native phosphates and arseniates that we possess, from which it follows that a metal may give with each of those two acids no less than eleven different salts. Mineralogy presents us with nine different modes of combination, and the remaining two are very common in our laboratories.

In an old memoir on a particular species of tantalite from Finland, the specific gravity of which much exceeded that of the other species, and whose powder had a bright cinnamon colour, I had determined it to be a *tantaliuret of iron* (not oxidated) mixed with a small portion of tantaliate of oxidulous iron. The new experiments which I have lately made on tantalium prove that what I formerly considered as metallic tantalium, is in fact an oxide of that metal which I have since learnt how to reduce completely. We thus have two tantalites in Finland, one of which is the neutral tantalite of the protoxides of iron and manganese, and the other a compound of the protoxide of iron and protoxide of tantalium.

M. Mosander has been engaged in the analysis of the white serpentine, which we discovered at Gullsjo, on our visit to that place. You will find the formula in the systematic enumeration. This stone contains besides a little strontita.

M. Walmstedt has communicated to the Academy of Sciences a long work on Peridot; from which it follows that the formula is $\left.\begin{matrix} M \\ f \end{matrix}\right\} S$. He found no lime in that mineral.

Article VI.

On finding the Longitude at Sea. By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Great Ouseburn, Nov. 23, 1825.*

On account of the importance of the subject, various expedients have been devised. Of those which are perfectly just in theory, few can be employed on account of the peculiar circumstances in which the mariner is placed. Notwithstanding the number of plans that have been adopted, and the great excellency of many of them, the problem has been but imperfectly solved, and perhaps no very great improvement can be expected. However, whatever tends to increase the accuracy of the methods now in use, or to facilitate their application, must be considered important both in a scientific and commercial point of view.

The lunar method, although less accurate than some others, is that on which the mariner must chiefly rely; it is used in our navy, and on board the East India ships; it gives good practical

results; but since the computation is tedious, and since some skill in observing, as well as in the use of nautical tables, is required, it is rarely adopted by the masters of merchant vessels: indeed the merchant service is left without any ready means of determining the longitude; it is deduced from the reckoning which depends upon the log only, and the longitude is erroneous frequently as much as 2°, 3°, or even 4°; and doubtless these errors occasion the loss of many lives and much property.

It is to be regretted that the owners of ships do not pay more attention to the instruments which are to guide their property, and many of their fellow creatures, during long and hazardous voyages; frequently the only instrument carried out, besides the compasses, is an octant, with plain sights; these are usually so carelessly fitted up by country workmen, that I have detected a very considerable index error, which was not noticed, nor known to exist, either by the workman or the master of the ship. So long as such instruments are employed, neither can the lunar method nor the chronometer be of any use; frequently these octants are fitted up without a tangent screw; and of these, in very common use, there are very many on which reliance cannot be placed nearer than two or three minutes. In the adjustment either in estimating or removing the index error, I have seen the workman observe any object, as a chimney corner, not 50 yards off; and with the great error which so crude an observation introduces, the instrument has been taken to sea as perfect.

The object of the present paper is to propose a more accurate measure of a lunar distance than can be taken with any sextant; to register the corrections of every possible distance, and under all circumstances, in tables, whereby all the labour of computation will be taken out of the hands of the master of a merchant ship, and the longitude will be found certainly within 30′ of a great circle; while the more scientific mariner will obtain more accurate results by directly working out every observation.

The lunar method is never employed in the merchant service, because the computation is long and tedious, and the masters of these vessels are seldom able to apply logarithmic tables: whoever will take the trouble to work out an example will find it occupy more time than the masters of ships in general can spare, and besides they are seldom possessed of sufficient knowledge. The sextant is so imperfect an instrument, that there is room for very considerable improvement; it is certainly a most convenient and valuable nautical instrument; but when the sun or moon is viewed, the image is fringed, on account of the passage of the light obliquely through the mirrors, whence two equally good observers will take different measures of the same angle. The radius of the instrument is too small to admit of any great accuracy; for on account of the principle of the instrument, it is equivalent to one which does not depend on the

reflecting principle of half the radius; if then its radius be nine inches, it is equivalent to an instrument of $4\frac{1}{2}$ inches radius, which is not on the reflecting principle. The greatest defect of the sextant, except it be constructed on Sir I. Newton's principle, which is decidedly the best, arises from the very low power of the telescope which is applied to it. The aperture of the telescope never exceeds one-half or three-fourths of an inch, and the power is generally about five or seven times. Now it is well known to all practical astronomers, that when the aperture of a telescope is very much reduced, the inflected light causes great indistinctness; small as is the aperture of the telescope, half of it only receives the rays which emanate from each object; the inflection which takes place at the termination of the silvered part of the horizon glass, and about the cell which includes the object glass of the telescope, renders the vision indistinct. Also the rays proceeding from one object suffer two reflections, and those from the other pass through a thick plate of glass placed obliquely; the consequence of this, and the very small aperture used, is, that the field of view is so dark, that stars are observed with great difficulty, and the horizon cannot often be seen in the night. Although the telescope may magnify six times, in which case an angle of 10″ will be just visible, I know from my own experience, and the testimony of others well versed in these matters, that on account of the darkness of the field of view in the night time, and the fringes attached to the objects viewed, a measure, correct to a minute, is a very good one.

What I have already stated will justify an attempt to render the lunar method more accurate, and to furnish the masters of merchant vessels with a method which shall remove all the difficulties which at present exist in working out the observations.

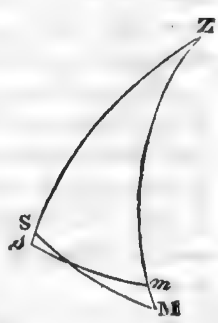
During a voyage to St. Helena, the great astronomer Dr. Halley proved that a telescope of five or six feet long might be easily managed in moderate weather, and that occultations of the fixed stars and planets might be observed; hence he proposed occultations to be used for finding the longitude; and since telescopes, magnifying 20 or 30 times, are now constantly used at sea, I do not anticipate any objection to instruments of the like power; those which Dr. H. proved might be used, magnified much more. A more correct value of the longitude cannot be obtained than that deduced from an occultation, and it is to be regretted that they have not been more generally used, and that so few have been noticed in the Nautical Almanac; for they happen so frequently, that even with the aid of a good watch, the longitude might always be known to half a degree; and by them the rate of a chronometer may always be ascertained.

Instead of measuring the moon's distance from a star when that distance is great, I propose the use of a refracting telescope,

having a divided object or eye glass micrometer; the latter will resemble the "Coming up glass," invented, I think, by Jones. With this instrument I propose to measure the moon's distance from a star, when this distance does not exceed 2°, or 2° 30′. The advantages of this method are, that the distance will be much more correctly measured than by any sextant (Dr. Halley has proved that a telescope of sufficient power may be used, and instruments of adequate power are daily employed at sea); and the limits of the observations are such, that the correction to be applied to the observed, to reduce it to the true distance, may be registered in tables of short compass in every case, which will take out of the hands of the most ignorant master of a ship all the labour and uncertainty of the calculation, and enable him to ascertain his longitude certainly to half a degree; whereas at present he is uncertain to 3° or 4°, owing to which there is a serious loss of both life and property. The observation will be better taken than by the sextant, because neither the magnifying power nor light of the telescope is limited, and the images are as well defined as in any telescope.

This method is peculiarly adapted to the merchant service, because the observation is easily made, and the whole calculation will be limited to two or three common rule-of-three statements. The data or principles on which the tables will be constructed are as follows:—Let Z be the zenith, M the apparent place of the moon, S that of the star. By a sextant, or other proper instrument, measure the altitudes of the two bodies; from the zenith, and through the moon and star, pass two great circles; Z M = 90° − ☽'s apparent altitude; Z S = 90° − ✱'s apparent altitude; correct the apparent altitude of the moon for refraction and parallax; then Z *m* = 90° − ☽'s true altitude; correct the stars for refraction, and Z *s* = 90° − ✱'s true altitude. S M is the apparent distance, measured by the telescope and micrometer; and *s m* is the true distance which is required. Therefore in the spherical triangle Z S M, the three sides are known by measurement, and the angle Z is first required; log.

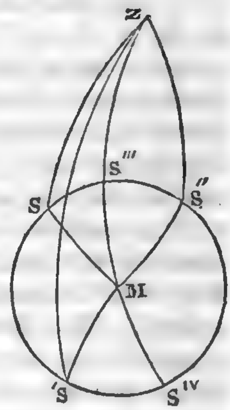
$$\cos.\tfrac{1}{2}z = \tfrac{1}{2}\Big\{\text{log. cosec. Z S} + \text{log. cosec. Z M} + \text{log. sin.}\tfrac{1}{2}\{\text{Z S} + \text{Z M} + \text{S M}\} + \text{log. sin.}\Big\{\tfrac{1}{2}\{\text{S Z} + \text{Z M} + \text{S M}\} - \text{SM}\Big\}\Big\}$$



Refraction and parallax take place entirely in vertical circles; refraction depends upon the apparent altitude only; and although it requires a correction for the thermometer and barometer, it may be neglected on account of its minuteness; however, when the observer has ability and leisure, since most large vessels are provided with a marine barometer (and none should be unprovided with one,

because it gives indications of a gale considerably before its arrival), the correction may be easily applied, and will add to the accuracy of the observation. But in general the common tables of refraction and parallax will suffice.

Having applied the corrections, and found the angle Z, which angle is not altered by the corrections, the next step is to find the arc $s\,m$; and the difference, viz. $s\,m$ — S M, or S M — $s\,m$, is the quantity to be registered in the tables, and which is to be added to or subtracted from S M, the measured distance, to reduce it to the true. In the spherical triangle, Z $s\,m$, Z s, Z m, and $\angle$ Z, are given; $s\,m$ is required. To the nat. v sin. of the difference of the sides, add the number corresponding to the log. v sin. of the angle z + log. sin. Z m + log. sin. Z s; the sum is the nat. v sin. $s\,m$. These are the data on which I mean to construct the tables.



Let Z M be a zenith distance of the moon, the distance M S being about one-fourth part less than the diameter of the field of view of the telescope; with radius M S, and from M, describe a small circle S S′ S″. Suppose M S = 2°; Z S‴ = Z M — 2°, and Z S^{IV} = Z M + 2°; and the other values of Z S, Z S′, Z S″, are intermediate; to wit, Z S‴ = Z M — M S; Z S = Z M — Z S + 10′; Z S′ = Z M — Z S + 20′, &c. For all these cases, the correction will be registered, which, added to or subtracted from M S, will reduce it to the true distance, or clear it from the effects of refraction and parallax. Diminish M S by 15′, Z M and Z S having the same values as before; and calculate the corrections until by continually subtracting 15′, M S becomes equal to the moon's semidiameter. The following is a specimen of two pages of the tables.

Page 1. Z M = 5°. Page 2. Z M = 6°.

Z S (° ′)	Values of S M. 15′	20′	30′	40′	&c.	to 2° 0′	Z S (° ′)	Values of S M. 15′	20′	30′	40′	&c.	to 2° 0′
3 00	— a	— b					4 00	— e	— f				
3 10	— c	— d					4 10	— g	— h				
3 20							4 20						
3 30							4 30						
3 40							4 40						
&c. to							&c. to						
7 00	+ i	+ k					8 00	+ l	+ m				

At the head of each page stands a value of Z M, which commences at 5°, and increases by 1° in each page to 75°; the horizontal column contains values of S M, beginning at 15′, next 20′, next 30′, as far as 2°; the lateral column contains values of Z S; the first is Z M−2°, the last = Z M + 2°; the intervals are 10′: in the spaces where the values of S M and Z S meet, will be inserted the correction of the apparent distance, with its proper sign; thus required the correction, when

$$\begin{aligned} ZM &= 5^\circ \;\; 0' \\ SM &= 0 \;\; 20 \\ ZS &= 3 \;\; 10 \end{aligned}$$

In the space where the correction will be inserted, is found − *d*.

If the numbers be between those inserted in the tables, the correction is found by a simple proportion: suppose Z M = 6°, Z S = 8°, S M = 17′; 5′ : 2′ :: *l*−*m* : the proportional part to be applied to *l*. Beyond two or three such proportions, there will be no computation required. The tables will not give so very minutely accurate results as the direct solution of every case; but their deviation will never exceed 30′; I may safely say, 15′ of longitude. The parallax used in finding the tabulated corrections will be the mean; when, therefore, the proper correction is found, say mean parallax : true parallax :: tabulated correction : true correction.

A short subsidiary table may be constructed to make the correction for variations in the parallax more accurately, or it may be joined to the larger table, inserted between its columns.

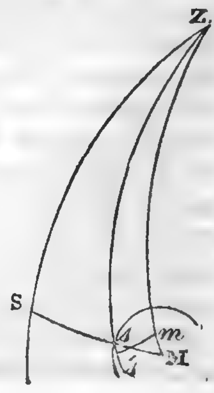
The tables will contain only 70 pages; viz. from 5° of zenith distance to 75°: in each horizontal line will be 12 spaces, or values of S M; and in each vertical column 25 values of Z S.

There are about 54 zodiacal stars sufficiently bright for the purpose; in this list are only nine of the 3.4 and 4.3 magnitude; all the rest, viz. 45, are 1.2, or 3. Stars of 3.4, 4.3, and even lower magnitude, may be readily observed. When close to the moon, by a telescope of moderate aperture and power; the common refracting telescope, not achromatic, is the best instrument. With an aerial of 18 feet focal length, varying the aperture from 1·3 to 2·5 inches, and without eye-hole, or any other addition, to exclude foreign light, I have observed many occultations of stars of the 3.4 and 4.3 magnitude, and even smaller. With small instruments of the same sort, I see small stars close to the moon better than with an achromatic of greater aperture. The telescope of my quadrant is a simple refractor, of 36 inches focal length, and one inch aperture; that of my transit instrument is an achromatic, 22 inches focus, and 1·5 aperture. With the former I see small stars better than with the other; indeed many can be seen through it, which the other does not render

visible using equal powers. Besides, in these observations, the simple refractor possesses one great advantage: when the moon, a bright star, or planet, is in or very near the field of view, the field is less illuminated than that of an achromatic; this arises partly from the length of tube required, and partly from the imperfections in the surfaces of the best lenses; no polish is perfect; and when a ray of light falls obliquely upon a lens, a certain dissipation takes place at its surface: this is much greater when two or three lenses are combined than when but one is used; of this any one will be convinced by trying the experiment upon a single convex lens, and a compound object glass; in telescopical observations, the difference here noticed is very conspicuous.

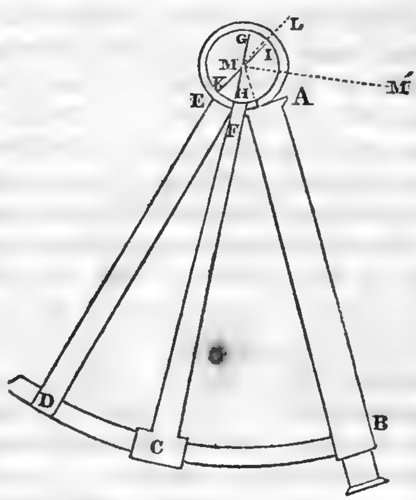
When a lunar distance is measured by the method here proposed, on account of the accurate measure obtained by the divided lens micrometer, the result will serve to rate the chronometer. Implicit reliance cannot be placed upon a chronometer, except means be at hand by which any changes that may take place in its rate of going can be discovered. When three chronometers are used, the results are more certain; but on account of their great expense, it is desirable to be able to correct one by celestial observations. The master of a merchant vessel may avail himself of the advantage by using the tables; and since the observations may be made almost daily, an instrument which will not cost a fourth the price of a chronometer will answer all its purposes: excellent watches, having a compensation balance, and going fusee, are made for less than 20*l.*; and if one of these be rated by a lunar observation, even but once in a fortnight, it will answer every purpose of a chronometer, which will cost from 80*l.* to 120*l.*; and if reliance be placed upon the chronometer alone, not less than three must be carried out.

There remains one other case of the lunar method of the greatest value, which has been too much neglected; that is, an occultation by the moon. Telescopes of sufficient power are used at sea; and Dr. Halley proved, during a St. Helena voyage, that they may be readily observed in tolerable weather. Were the occultations and small distances inserted in the Nautical Almanac, or other Ephemeris, the problem being but one case of the plan here proposed, the most correct possible measure of the longitude would result. As before, let Z be the zenith, M the centre of the moon, S the star: in the spherical triangle Z S M, S M is directly measured by the sextant, while Z S, Z M, are deduced from the observed altitudes: at the instant when the star touches the moon's limb, as at *s*; *s* M analogous to S M, is

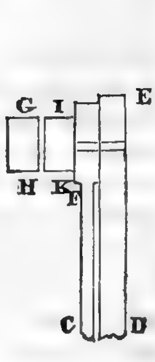


the moon's semidiameter, and since this is most correctly known, the distance of the moon's centre from the star will be ascertained with greater precision than by any measure: the time, which is a most important element, will be marked to half a second. Now apply the corrections; $s\,s'$, the effect of refraction upon the star; $M\,m$, the effect of parallax—refraction upon the moon; from which $s'\,m$, the true distance, results. Those who have leisure and skill will find this hitherto neglected method the most correct possible; and since an occultation may always be observed every two or three days, it will be sufficient for every purpose; or if taken less frequently, it will serve to rate the chronometer. For others, tables will be constructed, like those which have been already proposed. The values of S M will always be between the greatest and least semidiameters of the moon, increasing by intervals of 10″ or 12″; the maximum of Z S = Z M + ☽'s semidiameter; its minimum Z M − ☽'s semidiameter for every 3′ or 5′; Z M will increase by intervals of 3° or 5°, except near the zenith, when they will be 1° each.

The best time for taking the observations will be when the moon is not very near the full; for then the occultations may be observed on the dark limb.



Since it is difficult to measure the altitude of a star, on account of the excessive darkness of the field of view of the telescope applied to the sextant, I shall here propose a sextant, to which a telescope of any magnitude may be applied, and with which stars of even the sixth magnitude may be observed; and with which the horizon will be distinctly seen, when the night is too dark to allow the common sextant to be used. A B is a telescope attached to one of the radii of an octant or sextant; F C the index, which is mounted in the common way. G H is a plain speculum, fixed perpendicularly to the index; another plain speculum, I K, is supported by a strong arm to the frame of the sextant, and perpendicular to its plane. The disposition of the mirrors is shown in fig. 2. First, set the mirrors perpendicular to the frame, inclining the fixed mirror I K to the axis of the telescope, so that it may reflect down it a ray L M, the angle L M B being about 100° or 110°. Bring the



index to 0°, 0′, 0″, and bring the index mirror G H into the same plane with the fixed one, I K. Hold the plane of the instrument perpendicularly to the horizon, and in such a position that the horizon of the sea is received through the telescope. Let M′ be the moon, or a star; move the index till this object is seen through the telescope. As in the common sextant, the rays from both objects pass down the axis of the telescope, and therefore the images are in contact. The advantages are that the telescope is not limited in size; the mirrors being very near the object lens, less light is lost than when more remote, as in the sextants in common use; the images do not suffer the excessive loss occasioned by two reflexions; the images are very bright and very distinct; the oscillation is half that of the sextants in common use. There is far less difficulty in finding small objects than might be supposed; and stars of even the sixth magnitude are easily observed. If the circle be completed it makes an excellent repeating circle; it is infinitely easier to find a star with it than with the common repeaters, because each of the objects must alternately suffer two reflections; and upon the proposed plan, the instrument does not require alternate inversions. If a crystal be substituted for one mirror, it makes an excellent goniometer. Should this form of the sextant be not approved, the Newtonian may be recommended as having some great advantages; a larger and more powerful telescope may be employed than can be applied to those in common use, and can be more permanently secured; the mirrors are much nearer to each other and to the telescope: on this account, it gives brighter and better defined images.

I shall have great pleasure in presenting the requisite apparatus to any officer in the navy, and to one in the East India Company's service, who will take the trouble of observing with it during a voyage; requisite calculations and tables will be provided for their use; requesting that in return these gentlemen will furnish me with the observations they make, and comparative results from the chronometer, and the lunars, taken on board their respective vessels.

I remain, Gentlemen, yours very truly,

J. B. Emmett.

Article VII.

On the Measurement of Heights by One Barometer, with the requisite Tables. By Mr. J. Nixon.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Leeds, Nov.* 1825.

It is so rare to have at command in the course of a series of barometrical observations the uninterrupted co-operation of an intelligent friend, and so hazardous to the safety of the instruments as well as to the accuracy of the observations, to entrust them to strangers, that most generally recourse will necessarily be had, especially by the geologist and botanist, to the less accurate and more tedious method of ascertaining the altitudes independent of the observed contemporaneous height of the barometer at the lower station. To point out such a plan of operations, and to furnish such tables as, it is presumed, will insure to the method all the accuracy and facility of which it may be susceptible, is the proposed object of the present article.

The description of barometer best adapted for the labours of the geologist, &c. will be that of Sir H. Englefield. The mercurial column should be uniformly bright, perfectly continuous, and unstudded with air-bubbles, or hazy *nebulæ.* The vacuum above being perfect, a very gradual inclination of the tube will suffice to produce a smart blow the moment the column comes in contact with the summit of the tube; otherwise the concussion will be dull and nearly inaudible; the mercurial column will occasionally appear broken or divided, and will rarely measure, on being disturbed or exposed to a different temperature, precisely the same corrected height.

The scale, fixed parallel to the tube, should be engine-divided (not enamelled) into inches and twentieth parts. The vernier (nonius or index), comprehending a space of 1·2 inch subdivided into 25 equal parts, is more accurately moved by a milled-head nut and endless screw than simply by the hand. A brass ring or hollow cylinder attached to the vernier embraces the tube, and has its (base, or) under edges perpendicular to the mercurial column, and precisely of the height of zero of the vernier. Barometers furnished with a triangular notch of brass in lieu of the ring subject the observer to the risk of measuring the height of the column as affected by parallax.

The scale must be so affixed that its zero may coincide with the convex surface of the mercury within the *tube,* the pressure being 0. This is effected by raising the scale a quantity equal to the correction for capillarity subtracted from the neutral point divided by the capacity *plus* 1.* The capacity being 1-30th,

* The method given vol. x. p. 170, is not *strictly* accurate.

divide the neutral point (31 inches) by 30 + 1, and deduct the capillarity (0·100) from the quotient (1·000). The scale being elevated by the remainder (0·900), the observed heights will be proper for calculation without correction for capacity or capillarity. When the alteration cannot be made conveniently, deduct from the observed pressures a *constant* quantity found as just indicated.

In narrow tubes the mercury will not move so freely as in those of larger diameter. On the other hand, as the latter are more liable to be broken, it will be advisable to make choice of one with a capacity of about 1-50th or 1-60th.

The thermometer, divided into single degrees of Fahrenheit's scale, should be well graduated, having its fixed points accurately laid down. Should the thread of mercury ever appear divided, the instrument may be rejected as unserviceable. In observing the height of the thermometer, the eye must be held perpendicular to the summit of the column, and at a moderate distance from it.

A light tripod, similar to that of a theodolite, on which to suspend the barometer, is absolutely indispensable. To the tripod should be added as an occasional substitute a rod of iron, nearly of the diameter of the suspension ring of the barometer, and about 15 inches in length. It should be turned up a little at one end, and flattened and pointed at the other, to admit of its being driven into the seams of rocks, walls, &c. On many occasions a 12-inch telescope, fitted up with adjustable cross-wires and ground spirit level, will be found to be extremely useful.

In possession of the requisite instruments, the observer will naturally inquire, what are the most favourable circumstances as to pressure, temperature, &c. under which the observations may be made?

In the first place, as the pressures observed at the base of the mountain before and after the ascent, serve, together with the elapsed time, as data whence to obtain by interpolation the pressure at the time of the observation on the summit, the most favourable period as to pressure, will be when the barometer is stationary, or nearly so, and at its mean height for the place.* In this favourable condition of the atmosphere, the pressures at equal altitudes will most probably be equal over a considerable extent of country, and the variations at the base in the interval of the two observations insignificant, and what is of more importance, and may rarely occur in opposite circumstances, sensibly uniform. The horizontal distance of the stations being inconsiderable, a maximum pressure may be preferable, the

* To the pressure expressed in thousandth parts of an inch, add the height in feet of the place of observation above the sea, which will give the pressure at that level wi h sufficient exactness. The mean pressure at the sea is considered to be 30·000 inches

density of the air approaching more nearly to that of the mercury.

In the second place, as the pressures are very irregular about sunrise and sunset, the observations should not commence before the sun has risen an hour or two, and must terminate when practicable at nearly an equal distance of time previous to his setting.

As to temperature, cold weather would appear to be most eligible, the variable correction for humidity being unimportant, and the density of the air differing less from that of mercury than at more elevated temperatures. On the other hand, as the barometrical experiments whence the principal coefficients of the formula have been derived were made at temperatures some little above a mean, it will be most prudent to avail ourselves of calm cloudy days, with the thermometer steadily between 50° and 60° F.

The observer will consequently avoid as much as possible low, fluctuating, or rapidly varying pressures; high and especially changeable winds; also extremely hot weather, particularly if extraordinarily dry or damp. Cold winds, with a hot sun, are decidedly objectionable; the difficulty of ascertaining the true temperature of the mercury, or even that of the air, being almost insuperable. Extremes of temperature occurring in the course of the observations are very untoward.

At the level of the sea the heights of the barometer will remain unchanged under every vicissitude of temperature of the atmosphere. Stationed on an eminence the mercury will rise and fall with every accession and diminution of heat affecting the subjacent stratum of air. Hence the impossibility of ascertaining in elevated situations the correct rate of the variation of the pressure, and the absolute necessity of placing the inferior barometer, whatever may be the differences of level required to be measured, in the very lowest situation in the vicinity, in order to obtain this indispensable datum with the requisite accuracy. In making the consequent calculations, it will also be necessary to compute, in the first instance, the altitudes of the different superior stations above this inferior or reference station, whence their relative heights may be subsequently obtained by the mere subtraction of the one from the other.

When the reference station cannot be conveniently revisited subsequent to the ascent, a second observation should follow the first after the lapse of at least half the time required to gain the summit. Again, on reaching the base of the mountain in some other direction, or on arriving at some eligible low situation, the rate of the variation of the pressure may be deduced from two or more observations made at intervals of an hour or two. By taking a proper mean of the rate at the two places, the pressure at the reference station at any given time may be inferred with

tolerable accuracy. In order to ascertain the rate of the fall or rise of the barometer, the pressures must be first reduced to one and the same temperature; preferably that of the mercury at the first observation at the reference station.

One datum more is wanting,—the temperature at the base contemporaneous with that observed at the summit. To recur to interpolation, as in the case of the pressure, would evidently be purely conjectural, and most generally incorrect, the observer being engaged at the upper station in the heat of the day. A register of the thermometer at brief intervals must consequently be kept either at the reference station, or somewhere in the immediate vicinity nearly on the same level. The thermometer being suspended the height of the eye in a northern aspect, to which the wind may have free access, any one of moderate capacity (furnished with a watch) will succeed in noting its indications to the accuracy of at least a degree, whence the temperature at the base for any given instant may be readily deduced.

Unfurnished with a similar register, substitute in the calculation the double of the temperature at the upper station for the *sum* of the detached thermometers, and increase the resulting altitude by the *square* of $\frac{1}{500}$th part thereof. When the difference of level does not exceed 3600 feet, the augmentation may be found in Table V.

The method of making and registering the observations remains to be indicated.

The mercury being forced by means of the screw pressing against the leathern bag of the cistern to within a quarter of an inch, but on no account to the very summit of the interior of the tube (which would force part of the mercury through the pores of the leather), the barometer may be carefully inverted, and carried, lightly grasped in the hand, with the cistern *always* uppermost, at an angle of about 45° with the horizon. It should be kept at some distance from the body, and, when practicable, on the shaded side, the suspension end projecting in front, except on climbing steep acclivities, when it will be more safe to have it in the rear. On making a halt, avoid placing the instrument on damp ground, or on rocks exposed to the rays of the sun.

Arrived at the station, erect the tripod, the legs being firmly fixed in the ground, or rendered steady by piling heavy stones around them. A wall, rock, tree, &c. being near the station, make use of the iron crook in preference to the tripod, and drive it firmly into the shaded side of the object at the proper height. When the situation is open to the direct rays of the sun, or when the shaded side of the rock, wall, &c. is so much exposed to the fury of the wind that the crook must be inserted on the sunny side, it will then be necessary to shade the barometer by one of

the legs of the tripod; by the interposition of the body, a pile of turf, stones, &c.; or by an umbrella properly held by the guide. In gusty weather the oscillations of the mercurial column may be checked by surrounding the cistern end with sods, peats, &c. care being taken to preserve the instrument vertical. In tempestuous weather it will sometimes be found impracticable to make use of either tripod or crook. Then let the observer descend the leeward declivity of the mountain until the guide gives notice that the bubble of the telescopic level, pointed at the summit, is stationary in the middle. Mark the precise spot on which you stood, and continue the descent until the eye becomes level with the mark. Then by multiplying the height of the eye by the number of the levellings, the total fall is ascertained; and here erect the tripod, or fix the crook.

The summit being an extensive plain, let the observer seat himself on a rock or bank, and holding the barometer firmly between his knees, render it perpendicular to some distant level ridge, or other horizontal plane. The instrument may now be drawn nearer or made to recede, until the mercury stands at the lowest, and then will the column be perpendicular to the horizon.

When the cistern of the barometer does not coincide in height with the station, but is placed on higher or lower ground, be careful to register a remark to that effect. The difference of level not exceeding five or six feet, one foot answers so nearly to ·001 inch of mercury at any pressure, that it will be sufficiently correct, and more convenient to add to (or deduct from) the observed height of the barometer ·001 inch for every foot it may be suspended above (or below) the station.

The instrument being securely suspended on the crook or tripod, unscrew the bag as far as it will admit, and tap the sides of the barometer pretty freely. After a lapse of fifteen minutes in cloudy weather; but of double the time when the solar radiation is considerable, draw aside the brass tube concealing the interior thermometer, &c. and having noted within a parenthesis ( ) its correct height, place it in the shade fairly exposed to the air, but not to a partial current, at the height of five or six feet from ground that has not been recently exposed to the direct rays of the sun, and does not appear to be exceptionably damp.

To find the height of the barometer, keep it steadily vertical with the left hand, and seizing the milled head nut with the right, gradually lower the cylindrical ring, the eye being continued in a line with its under edge in front of, and behind the tube, until the light seen through the narrow slit *above* the convex summit of the mercurial column is just excluded. Quitting hold of the instrument observe that the contact continues good, or repeat the adjustment. When the instrument is in a narrow glen with dull objects immediately beyond the slit, fix a sheet of white or

(better) yellow paper behind the barometer, or let the guide interpose his hand held at the distance of about a foot.

Having completed the adjustment of the rim of the vernier to the height of the column, note immediately the indication of the detached thermometer, and the time of observation, adding 12 hours to the times, p. m.

In reading off the pressure, write down first the inches and tenths the next below the zero of the vernier, to which affix the remaining fractional parts as indicated by the vernier. To read these off correctly, commence at the zero of the latter,—terming it ·050 when a twentieth of an inch intervenes between it and the tenth of an inch already registered,—and count the lines upwards, calling each ·002 inch, until one of the lines of the vernier coincides in height with, or appears to be a prolongation of, some one of the adjoining lines of the fixed scale of inches.

Having completed the requisite observations at the summit station, the altitudes of several of the surrounding mountains, &c. may be determined after the following method on making the descent to the reference station.* Selecting the loftiest of the objects visible below the apparent horizon, steadily bisect its summit from time to time with the intersection of the cross wires of the telescope until the bubble of the level is observed by the guide to remain in or near the middle. Then fix the tripod for a support, and continue to alter its height until the bubble of the telescope resting upon it, and accurately pointed at the object, remains steadily between the marks. Here suspend the barometer, and make the requisite observations of the pressure, temperature, &c. allowing for the height of the tripod above the cistern. With these data, in addition to those obtained at the reference station, we learn by calculation the elevation in feet of the level point above the latter. To this difference of level *add* the square of the distance, in miles of the observed object, from the level point multiplied by 0·576 foot,† and we have the correct altitude of the object above the level of the reference station. The telescope being small, and the distance taken merely from the map, the observations should not extend to mountains, &c. at a greater distance than eight or ten miles.

In mountainous districts, consisting of rocks regularly stratified under a moderate dip, the depths and inclination of the principal beds may be ascertained with tolerable precision by barometrical measurements made in the ravines, beds of torrents, or on the precipitous declivities of the mountains. The first object in an undertaking of this description will be to determine with great care the relative heights of a sufficient number of

* De Luc.

† $= 5280 \text{ feet} \times \text{tang.} \left(\frac{52''}{2} - \frac{52''}{15}\right)$. A mile (5280 feet) subtends an arc of 52″; the refraction is considered to be 1-15th of the arc.

reference stations judiciously selected on the banks of the principal streams irrigating the district. The absolute elevation of the upper surfaces of the different strata above the *lowest* of these stations being computed from the barometrical observations, the height of some one of the principal or best identified beds, at the various places where sections have been measured, may be marked on a map, accurately laid down on a large scale. To find the dip, draw the direction line through a number of section-stations where the stratum is on the same level, or equally elevated above the lowest reference station, and is at its maximum height. From the remaining section-stations draw lines intersecting the direction line at right angles, and measure the distances in feet on the scale of the map. Dividing the distance by the difference of altitude of the stratum at the section-station and at the direction line, we obtain the tabular tangent of the dip in the vicinity of that section.

As the exactness of the barometrical measurements of the sections will depend almost entirely on the correct observation of the height and temperature of the mercurial column, the observer must so habituate himself to the accurate adjustment of the ring of the vernier to the proper height that the pressures may be read off true to ·002 or ·003 inch. To meet the other more serious difficulty, the observations should be made exclusively in calm cloudy weather, between noon and three, p.m. at a temperature little differing from the mean annual one.

Having completed the observations at the reference station, proceed to the torrent, and slowly ascend to the summit, marking with piles of stones the heights of the planes of contact of the strata; cautiously avoiding to mistake an accumulation of bowlders for an alternation of the beds. When the contact is indiscernible in the ravine, &c. deviate to the right or left on the same level, when it will seldom fail to be recognized in a scar, cliff, &c. or identified by the gushing out of numerous springs at the same altitude. In most formations, the change of beds is generally intimated by the varied verdure of the vegetation; the appearance of different plants, &c. Descending to the reference station, suspend the barometer at every pile of stones, noting its height and that of the interior thermometer, after a lapse of time, *precisely* the same at every observation, or about ten minutes. When the beds are only a few feet in thickness, it will be better to measure them with the telescopic level, and a light staff divided into inches, than to recur to the barometer.

Construction of the Tables.

The reduction of the pressures at the reference station to one temperature, the determination of the rate per hour of the fall or rise of the barometer, together with the interpolation of the pressures and temperatures to the times at the summit, present

a formidable list of calculations merely preparatory. Independent of the objectionable labour, the reductions themselves will ever be of questionable accuracy; for should the variation of the pressure, considered as constant, have been subject to irregularities in the interval of the observations, the interpolations will most probably be incorrect. This remark, it is true, is inapplicable to the register of the thermometer, yet when the altitude has been computed, in default of such register, with twice the temperature at the upper station considered as the sum of the thermometers, the given correction will be exact only when the temperature declines 1° for an ascent of 250 feet. Hence the probable inutility on the one hand of introducing minute corrections in the calculations, and the necessity on the other of forming tables, so constructed after approximative formulæ, as to render the computations simple, brief, and easy.

The barometrical observations of Shuckburgh, Roy, and Ramond, made at ordinary temperatures on mountains, having on the average an elevation of 3000 feet, coincide in determining the coefficient or multiplier of the logarithmic difference of the pressures to be about 64096 at 60° F. If we suppose the barometer at the base to be 30·000 inches, and the temperature of the air 65°, we shall have the pressure at the summit equal to 26·940 inches, the thermometer indicating about 55°. The mean of nearly 300 afternoon observations, made by Mr. Daniell within a thermometric range of from 60° to 70° (mean 65°) give the corresponding dew point at 53°. The point of condensation at the summit may be estimated, according to the experiments of Mr. Dalton, at 48°. The mean density of the intercepted column of air would consequently be equal to that of perfectly dry air of the temperature of 62·7.* Hence the coefficient at the level of the sea would be equal to $60095 + \frac{30\cdot7}{480} = 63939$; or, corrected for the vertical diminution of gravity, 64119. The result differs so little from 64096, that we are warranted in concluding the coefficient of dry air at 32° F. to be, as inferred from the experiments of Arago and Biot on the relative weights of mercury and air, very nearly 60095.

The mean temperature of the stratum of air being 32°, we may estimate the corresponding dew point at 22°, the density being in such case equal to dry air at 33·3, for which the coefficient is 60258 at the level of the sea, or 60418 at an altitude of 3000 feet. In the formula of Laplace, it is given at 60345: Roy and

* Lower station 30·00 in. pressure,	65° air,	53° dew point,	2·8 equation.
Upper station 26·94	55	48	2·6
Mean	60		Mean 2·7
Equation	2·7		
	62·7	(See Table I. vol. x. p. 177.)	

Shuckburgh estimate it, as might be anticipated from their ignorance of the laws of the dilatation, &c. of moist air, considerably in error, stating it to be 60000.

Being, however, assured by Laplace that his formula agrees with the *ensemble* of the excellent observations of Ramond, we may venture to adopt it as the basis of the tables. Calling X the difference of level, H the height of the barometer, T that of the *centigrade* thermometer at the lower station, and h, t, the analogous quantities at the upper one, the pressures being corrected for difference of temperature of the mercury, the formula gives

$$X = 60345 \text{ ft.} \{1 + \cdot 002837 \cos. 2 \text{ lat.}\} \left\{1 + \frac{2(T+t)}{1000}\right\} \log. \frac{H}{h}.$$

Adapted to the more convenient zero of Fahrenheit's scale, the formula becomes

$$X = 56054 \{1 + \cdot 002837 \cos. 2 \text{ lat.}\} \left\{1 + \frac{T+t}{836}\right\} \log. \frac{H}{h}.*$$

If we conceive a number of barometers so stationed one above another that they shall indicate, in an atmosphere uniformly of the temperature of 0° F. equal differences of pressure of the value of ·01 inch, we may find the perpendicular height of the upper one h', standing at 21 inches, above the level of those beneath it, by the formula

$$56054 \text{ feet, } \log. \frac{H}{21{\cdot}00 \text{ inches}}. \quad \text{(See Table II.)}$$

The resulting heights with the corresponding pressures from 21·00 to 31·00 inches, being arranged in a table, the difference of level of any two of the inferior barometers, of which the pressures are given, is obtained by mere subtraction.

The pressures being generally expressed in thousandth parts of an inch, the value *additive* in feet of the last figure (from ·001 to ·009) may be found and tabulated by multiplying it by 24·345 divided by the required pressure. (See Table III.)

* Adopting the Daltonian theory of the constitution of the atmosphere, the correction for humidity will be very simple. We have but to subtract from the pressures (reduced to 32° F.) the force of the vapour corresponding to the observed dew point; and make the calculation after the following formula for an atmosphere of perfectly dry air, viz.

$$x = \log. \left(\frac{H-F}{h-f}\right) . 56220 . (1 + 002837 . \cos. 2 \text{ lat.}) \left(1 \quad \frac{T+t}{894}\right) \left\{1 + \frac{R+r}{a}\right\},$$

wherein R is the elevation of the lower station, and r that of the upper one above the level of the sea; a, the earth's radius in feet; F the tension of the vapour at the base; and f its force at the summit.

When F is to H as f is to h, the pressure of the atmosphere of vapour on the mercury does not affect the calculation, and we may use $\frac{H}{h}$ or $\frac{H-F}{h-f}$ indifferently. The ratio of f to h being greater (or less) than that of F to H, the altitude computed with $\frac{H}{h}$ in lieu of the pressures diminished by the tension of the vapour, will err in defect (or excess).

To make the correction for the difference of temperature of the mercury also additive, we have but to suppose that of h' to be at 100° F. Consequently as the interior thermometer of any one of the inferior barometers will never indicate so elevated a temperature, its perpendicular distance from h', as given in Tables II. and III. must be augmented by multiplying the difference of its attached thermometer, and 100° by 2·18 feet (the value of 1° of the difference at 0° F.). Tabularly arranged, the correction at 100° F. will be 0 feet; at 99°, 2·18 feet, &c. (See Table IV.)

The height of h' above the level of h and H being obtained by adding together for each the quantities given in the three tables, their difference will be equal to the elevation of h above H at 0° F. Multiplying this approximate height by the sum of the thermometers, and dividing the product by 836, we have the correction in altitude additive for temperatures above 0° F.

Multiplying ·002837 by cosine of twice the latitude, we obtain the fractional correction in altitude proper for that parallel. Then as an addition to (or subtraction from) the sum of the detached thermometers at 110°, equal to 1°, augments (or diminishes) the altitude ·001057, we have but to substitute for the fractional corrections their equivalents in degrees and quarters of the sum of the thermometers. At the equator, the equivalent would be $= \frac{\cdot 002837}{\cdot 001057} = 2\frac{3}{4}°$. (See Table V.)

Having given the altitude and height of the thermometer at the upper station, we find the mean temperature, the fall of the thermometer being 2° in 500 feet, by dividing the difference of level of the stations by 500, and adding the quotient, considered as degrees to the temperature observed at the summit. At 55° an increase of the mean temperature equal to 1° augments the altitude $\frac{1}{473}$. However as the fall of 2° in 500 feet may be considered in excess, we will call the mean dilatation $\frac{1}{500}$; the correction for altitudes computed with twice the height of the upper thermometer substituted for the sum of the thermometers, will then be equal to the square of $\frac{1}{500}$th part of the approximate altitude. (See Table VI.)

(*To be continued.*)

Article VIII.

Astronomical Observations, 1825.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825.	Stars.	Transits.		
Nov.	19.—α Piscium	22h	59′	47·35″
	19.—γ Piscium	23	08	10·15
	19.—κ¹ Piscium	23	18	02·56
	19.—Moon's First or West Limb	23	26	00·61
	19.—λ Piscium	23	33	11·68
	19.—19 Piscium	23	37	31·84
	19.—22 Piscium	23	43	05·22
	19.—ω Piscium	23	50	24·44
	21.—γ Pegasi	0	04	18·39
	21.—42 Piscium	0	13	27·35
	21.—110 Piscium	0	25	11·65
	21.—58 Piscium	0	37	59·00
	21.—Moon's First or West Limb	0	57	23·17
	21.—101 Piscium	1	26	30·82
	21.—104 Piscium	1	29	58·65
	22.—η Piscium	1	22	12·90
	22.—101 Piscium	1	26	31·10
	22.—103 Piscium	1	29	55·63
	22.—4 Arietis	1	38	47·71
	22.—Moon's First or West Limb	1	44	57·81
	22.—19 Arietis	2	03	37·20
	22.—29 Arietis	2	23	25·66
	25.—α Tauri	3	54	27·76
	25.—κ¹ Tauri	4	15	02·96
	25.—82 Tauri	4	17	44·48
	25.—Moon's East or Second Limb	4	22	45·09
	25.—τ² Tauri	4	31	50·96
	25.—243 Tauri	4	47	17·74
	25.—ι Tauri	4	52	44·11
	25.—105 Tauri	4	57	33·64
	25.—108 Tauri	5	05	02·56
	25.—η Tauri	5	08	51·63
	26.—τ Tauri	4	31	50·96
	26.—ι Tauri	4	52	44·54
	26.—105 Tauri	4	57	34·05
	26.—108 Tauri	5	05	02·77
	26.—η Tauri	5	08	52·05
	26.—Moon's East or Second Limb	5	18	23·01
	26.—ζ Tauri	5	27	17·84

Eclipses of Jupiter's satellites.

Nov. 15.	Immersion of Jupiter's fourth satellite	14h	13′	05″	Mean Time at Bushey.
		14	14	26	Mean Time at Greenwich.

N. B. Jupiter not well defined.

Nov. 17.	Immersion of Jupiter's first satellite	16	32	15	Mean Time at Bushey.
		16	33	36	Mean Time at Greenwich.
Nov. 24.	Immersion of Jupiter's first satellite	18	25	36	Mean Time at Bushey.
		18	26	57	Mean Time at Greenwich.
Nov. 25.	Lunar eclipse. Moon rose eclipsed. End	5	02	24	Mean Time at Bushey.
		5	03	45	Mean Time at Greenwich.

Article IX.

On new Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat. By M. Faraday, F.R.S. Cor. Mem. Royal Academy of Sciences of Paris, &c.*

The object of the paper which I have the honour of submitting at this time to the attention of the Royal Society, is to describe particularly two new compounds of carbon and hydrogen, and generally, other products obtained during the decomposition of oil by heat. My attention was first called to the substances formed in oil at moderate and at high temperatures, in the year 1820; and since then I have endeavoured to lay hold of every opportunity for obtaining information on the subject. A particularly favourable one has been afforded me lately through the kindness of Mr. Gordon, who has furnished me with considerable quantities of a fluid obtained during the compression of oil gas, of which I had some years since possessed small portions, sufficient to excite great interest, but not to satisfy it.

It is now generally known, that in the operations of the Portable Gas Company, when the oil gas used is compressed in the vessels, a fluid is deposited, which may be drawn off and preserved in the liquid state. The pressure applied amounts to 30 atmospheres; and in the operation, the gas previously contained in a gasometer over water, first passes into a large strong receiver, and from it, by pipes, into the portable vessels. It is in the receiver that the condensation principally takes place; and it is from that vessel that the liquid I have worked with has been taken. The fluid is drawn off at the bottom by opening a conical valve: at first a portion of water generally comes out, and then the liquid. It effervesces as it issues forth; and by the difference of refractive power it may be seen, that a dense transparent vapour is descending through the air from the aperture. The effervescence immediately ceases; and the liquid may be readily retained in ordinary stoppered, or even corked bottles; a thin phial being sufficiently strong to confine it. I understand that 1000 cubical feet of good gas yield nearly one gallon of the fluid.

The substance appears as a thin light fluid; sometimes transparent and colourless, at others opalescent, being yellow or brown by transmitted, and green by reflected light. It has the odour of oil gas. When the bottle containing it is opened, evaporation takes place from the surface of the liquid; and it may be seen by the striæ in the air that vapour is passing off from it. Sometimes in such circumstances it will boil, if the

* Philosophical Transactions for 1825, Part II.

bottle and its contents have had their temperature raised a few degrees. After a short time this abundant evolution of vapour ceases, and the remaining portion is comparatively fixed.

The specific gravity of this substance is 0·821. It does not solidify at a temperature of 0° F. It is insoluble, or nearly so, in water; very soluble in alcohol, ether, and volatile and fixed oils. It is neutral to test colours. It is not more soluble in alkaline solutions than in water; and only a small portion is acted upon by them. Muriatic acid has no action upon it. Nitric acid gradually acts upon it, producing nitrous acid, nitric oxide gas, carbonic, and sometimes hydrocyanic acid, &c. but the action is not violent. Sulphuric acid acts upon it in a very remarkable and peculiar manner, which I shall have occasion to refer to more particularly presently.

This fluid is a mixture of various bodies; which, though they resemble each other in being highly combustible, and throwing off much smoke when burnt in large flame, may yet by their difference of volatility be separated in part from each other. Some of it drawn from the condenser, after the pressure had been repeatedly raised to 30 atmospheres, and at a time when it was at 28 atmospheres, then introduced rapidly into a stoppered bottle and closed up, was, when brought home, put into a flask and distilled, its temperature being raised by the hand. The vapour which came off, and which caused the appearance of boiling, was passed through a glass tube at 0°, and then conducted to the mercurial trough; but little uncondensed vapour came over, not more than thrice the bulk of the liquid; a portion of fluid collected in the cold tube, which boiled and evaporated when the temperature was allowed to rise; and the great bulk of the liquid which remained, might now be raised to a comparatively high point, before it entered into ebullition.

A thermometer being introduced into another portion of the fluid, heat was applied, so as to keep the temperature just at the boiling point. When the vessel containing it was opened, it began to boil at 60° F. As the more volatile portions were dissipated, the temperature rose: before a tenth part had been thrown off, the temperature was above 100°. The heat continued gradually to rise, and before the substance was all volatilized, it had attained 250°.

With the hope of separating some distinct substances from this evident mixture, a quantity of it was distilled, and the vapours condensed at a temperature of 0° into separate portions, the receiver being charged with each rise of 10° in the retort, and the liquid retained in a state of incipient ebullition. In this way a succession of products were obtained; but they were by no means constant; for the portions, for instance, which came over when the fluid was boiling from 160° to 170°,

when redistilled, began to boil at 130°, and a part remained which did not rise under 200°. By repeatedly rectifying all these portions, and adding similar products together, I was able to diminish these differences of temperature, and at last bring them more nearly to resemble a series of substances of different volatility. During these operations I had occasion to remark, that the boiling point was more constant at, or between 176° and 190°, than at any other temperature; large quantities of fluid distilling over without any change in the degree; whilst in other parts of the series it was constantly rising. This induced me to search in the products obtained between these points for some definite substance, and I ultimately succeeded in separating a new compound of carbon and hydrogen, which I may by anticipation distinguish as bi-carburet of hydrogen.

Bi-carburet of Hydrogen.

This substance was obtained in the first instance in the following manner: tubes containing portions of the above rectified products were introduced into a freezing mixture at 0°; many of them became turbid, probably from the presence of water; one, received at 176°, (by which is meant that that was the boiling point of the contents of the retort when it came over) became partly solid, crystals forming round the side, and a fluid remaining in the centre; whilst two other portions, one received at 186°, and the other at 190°, became quite hard. A cold glass rod being introduced into one of these tubes, the mass within was found to resist considerable pressure; but by breaking it down, a solid part was thrust to the bottom of the tube, whilst a fluid remained above: the fluid was poured off, and in this way the solid portion partly purified. The contents of the tube were then allowed to fuse, and were introduced into a larger and stronger tube, furnished with another which entered loosely within it, both being closed of course at the lower end; then again lowering the temperature of the whole to 0°, bibulous paper was introduced, and pressed on to the surface of the solid substance in the large tube by the end of the smaller one. In this way much fluid was removed by successive portions of paper, and a solid substance remained, which did not become fluid until raised to 28° or 29°. To complete the separation of the permanently fluid part, the substance was allowed to melt, then cast into a cake in a tin foil mould, and pressed between many folds of bibulous paper in a Bramah's press, care having been taken to cool the paper, tin foil, flannel, boards, and other things used, as near to 0° as possible, to prevent solution of the solid substance in the fluid part to be removed. It was ultimately distilled from off caustic lime, to separate any water it might contain.

The general process, which appears to me to be the best for the preparation of this substance only, is to distil a portion of the fluid deposited during the condensation of oil gas, to set aside the product obtained before the temperature rises to 170°, to collect that which comes over by 180°, again separately that which comes over by 190°, and also the portion up to 200° or 210°. That before 170° will upon redistillation yield portions to be added to those of 180° and 190°; and the part obtained from 190° upwards will also, when redistilled, yield quantities boiling over at 180°, 190°, &c. Having then these three portions obtained at 180°, 190°, and 200°, let them be rectified one after the other, and the products between 175° and 195° received in three or four parts at successive temperatures. Then proceed with these as before described.

It will sometimes happen, when the proportion of bi-carburet of hydrogen is small in the liquid, that the rectifications must be many times repeated before the fluids at 185° and 190° will deposit crystals on cooling; that is to say, before sufficient of the permanently fluid part at low temperatures has been removed, to leave a solution so saturated as to crystallize at 0°.

Bi-carburet of hydrogen appears in common circumstances as a colourless transparent liquid, having an odour resembling that of oil gas, and partaking also of that of almonds. Its specific gravity is nearly 0·85 at 60°. When cooled to about 32° it crystallizes, becoming solid; and the portions which are on the sides of the glass exhibit dendritrical forms. By leaving tubes containing thin solid films of it in ice-cold water, and allowing the temperature to rise slowly, its fusing point was found to be very nearly 42° F.; but when liquid it may, like water and some saline solutions, be cooled much below that point before any part becomes solid. It contracts very much on congealing, 9 parts in bulk becoming 8 very nearly; hence its specific gravity in that state is about 0·956. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf sugar.

It evaporates entirely when exposed to the air. Its boiling point in contact with glass is 186°. The specific gravity of its vapour, corrected to a temperature of 60°, is nearly 40, hydrogen being 1; for 2·3 grains became 3·52 cubic inches of vapour at 212°. Barometer 29·98. Other experiments gave a mean approaching very closely to this result.

It does not conduct electricity.

This substance is very slightly soluble in water; very soluble in fixed and volatile oils, in ether, alcohol, &c.; the alcoholic solution being precipitated by water. It burns with a bright flame and much smoke. When admitted to oxygen gas, so much vapour rises as to make a powerfully detonating mixture

When passed through a red hot tube, it gradually deposits carbon, yielding carburetted hydrogen gas.

Chlorine introduced to the substance in a retort exerted but little action until placed in sun-light, when dense fumes were formed, without the evolution of much heat; and ultimately much muriatic acid was produced, and two other substances, one a solid crystalline body, the other a dense thick fluid. It was found by further examination that neither of these were soluble in water; that both were soluble in alcohol—the liquid readily, the solid with more difficulty. Both of them appeared to be triple compounds of chlorine, carbon, and hydrogen; but I reserve the consideration of these, and of other similar compounds, to another opportunity.

Iodine appears to exert no action upon the substance in several days in sun-light; it dissolves in the liquid in small quantity, forming a crimson solution.

Potassium heated in the liquid did not lose its brilliancy, or exert any action upon it, at a temperature of 186°.

Solution of alkalis, or their carbonates, had no action upon it.

Nitric acid acted slowly upon the substance and became red, the fluid remaining colourless. When cooled to 32°, the substance became solid and of a fine red colour, which disappeared upon fusion. The odour of the substance with the acid was exceedingly like that of almonds, and it is probable that hydrocyanic acid was formed. When washed with water, it appeared to have undergone little or no change.

Sulphuric acid added to it over mercury exerted a moderate action upon it, little or no heat was evolved, no blackening took place, no sulphurous acid was formed; but the acid became of a light yellow colour, and a portion of a clear colourless fluid floated, which appeared to be a product of the action. When separated, it was found to be bright and clear, not affected by water or more sulphuric acid, solidifying at about 34°, and being then white, crystalline, and dendritical. The substance was lighter than water, soluble in alcohol, the solution being precipitated by a small quantity of water, but becoming clear by great excess.*

* The action of sulphuric acid on this and the other compounds to be described, is very remarkable. It is frequently accompanied with heat; and large quantities of those bodies which have elasticity enough to exist as vapours when alone at common pressures, are absorbed. No sulphurous acid is produced; nor when the acid is diluted, does any separation of the gas, vapour, or substance, take place, except of a small portion of a peculiar product resulting from the action of the acid on the substances, and dissolved by it. The acid combines directly with carbon and hydrogen: and I find when united with bases forms a peculiar class of salts, somewhat resembling the sulphovinates, but still different from them. I find also that sulphuric acid will condense and combine with olefiant gas, no carbon being separated, or sulphurous or carbonic acid being formed; and this absorption has in the course of 18 days amounted to 84·7 volumes of olefiant gas to 1 volume of sulphuric acid. The acid produced combines with bases, &c.

With regard to the composition of this substance, my experiments tend to prove it a binary compound of carbon and hydrogen, two proportionals of the former element being united to one of the latter. The absence of oxygen is proved by the inaction of potassium, and the results obtained when passed through a red hot tube.

The following is a result obtained when it was passed in vapour over heated oxide of copper. 0·776 grain of the substance produced 5·6 cubic inches of carbonic acid gas, at a temperature of 60°, and pressure 29·98 inches; and 0·58 grain of water were collected. The 5·6 cubic inches of gas are equivalent to 0·711704 grain of carbon by calculation, and the 0·58 grain of water to 0·064444 of hydrogen.

Carbon	0·711704	or 11·44
Hydrogen	0·064444	1·

These quantities nearly equal in weight the weight of the substance used; and making the hydrogen 1, the carbon is not far removed from 12, or two proportionals.

Four other experiments gave results all approximating to the above. The mean result was 1 hydrogen, 11·576 carbon.

Now considering that the substance must, according to the manner in which it was prepared, still retain a portion of the body boiling at 186°, but remaining fluid at 0°, and which substance I find, as will be seen hereafter, to contain less carbon than the crystalline compound (only about 8·25 to 1 of hydrogen), it may be admitted, I think, that the constant though small deficit of carbon found in the experiments is due to the portion so retained; and that the crystalline compound would, if pure, yield 12 of carbon for each 1 of hydrogen; or two proportionals of the former element and one of the latter.

$$\left.\begin{array}{ll}\text{2 proportionals carbon} \ldots\ldots & 12\\ \text{1 proportional hydrogen} \ldots & 1\end{array}\right\} \text{13 bi-carburet of hydrogen.}$$

This result is confirmed by such data as I have been able to obtain by detonating the vapour of the substance with oxygen. Thus in one experiment 8092 mercury grain measures of oxygen at 62° had such quantity of the substance introduced into it as would entirely rise in vapour; the volume increased to 8505, hence the vapour amounted to 413 parts, or $\frac{1}{20 \cdot 6}$ of the mixture nearly. Seven volumes of this mixture were detonated in an eudiometer tube by an electric spark, and diminished in consequence nearly to 6·1: these acted upon by potash were further

forming peculiar salts, which I have not yet had time, but which it is my intention, to examine, as well as the products formed by the action of sulphuric acid on naphtha, essential oils, &c. and even upon starch and lignine, in the production of sugar, gum, &c. where no carbonization takes place, but where similar results seem to occur.

diminished to 4, which were pure oxygen. Hence three volumes of mixture had been detonated, of which nearly 0·34 was vapour of the substance, and 2·65 oxygen. The carbonic acid amounted to 2·1 volumes, and must have consumed an equal bulk of oxygen gas; so that 0·55 remain as the quantity of oxygen which has combined with the hydrogen to form water, and which with the 0·34 of vapour nearly make the diminution of 0·9.

It will be seen at once that the oxygen required for the carbon is four times that for the hydrogen; and that the whole statement is but little different from the following theoretical one, deduced partly from the former experiments. 1 volume of vapour requires 7·5 volumes of oxygen for its combustion; 6 of the latter combine with carbon to form 6 of carbonic acid, and the 1·5 remaining combine with hydrogen to form water. The hydrogen present, therefore, in this compound is equivalent to 3 volumes, though condensed into one volume in union with the carbon; and of the latter elements there are present six proportionals, or 36 by weight. A volume, therefore, of the substance in vapour contains

Carbon	$6 \times 6 =$	36
Hydrogen	$1 \times 3 =$	3
		39

and its weight or specific gravity will be 39, hydrogen being 1. Other experiments of the same kind gave results according with these.

(To be continued.)

Article X.

Analyses of Books.

Considerations on Volcanos, the probable Causes of their Phænomena, the Laws which determine their March, the Disposition of their Products, and their Connexion with the present State and past History of the Globe. By G. Poulett Scrope, Esq. Sec. GS. Phillips, 1825.

The author commences this work with a descriptive account of the phænomena of volcanos, remarking on their number and dispersion over the globe's surface, their division into *subaerial* and *subaqueous* vents, and the various conditions in which they at times appear, and which he classes as, Phases, 1. Of permanent eruption; 2. Of moderate activity; and 3. Of prolonged intermittences. Examples are given of these several phases, and a detailed description of their most ordinary characteristics.

In proceeding to account for these phænomena, he remarks that they prove the existence of a body of lava in a state of *ebullition* beneath every volcanic aperture during the period of its activity. He then examines the nature of this ebullition, and finds reason to conclude from the crystalline texture of all lavas, their sudden congelation on exposure, the small quantity of heat radiated by them even when incandescent, and the volumes of aqueous vapour they evolve, that their partial liquefaction is not owing to fusion, but to the vaporization of minute quantities of water interposed between the laminæ of their component crystals. The explosive escape of steam in vast quantity from the orifice of every volcano during an eruption, and from the surface and clefts of every lava current as it flows in open air, proves the existence of this fluid in the interior of the mass, and the elevation of liquid lava through the chimney of the volcano, the jets of scoriæ which accompany its rise, and the numerous vesicular cavities and pores of the rock into which it consolidates, all appear the necessary consequences of the generation of steam in this situation. Other aeriform fluids, perhaps many permanent gases, are probably evolved at the same time, but the great body of vapour appears to be aqueous.

The author proceeds to account for this process by supposing the continual accession from below of increments of caloric to a mass of crystalline rock of this nature already at an intense temperature, and confined beneath the solid crust of the globe. The consequence would be the increase of its expansive force without other limit than the yielding of the overlying strata. When this occurred, and a fissure was broken through them, and sufficiently enlarged, the consequent sudden reduction of pressure on the heated rock below would vaporize its contained water, liquefy the mass, and force it to rise in an intumescent or ebullient state to discharge itself through this aperture, while exploding volumes of steam escaping from its surface as rapidly as the weight and tenacity of the substance permits them, carry up into the air showers of red-hot fragments and ashes, in the exact manner of a volcanic eruption.

But the quantity of matter thus protruded by its weight and rapid congelation, tends to choke and seal up the fissure of eruption, and check the progress of the ebullition below; and in far the greater number of cases these obstructing causes sooner or later prevail, the force of *repression* obtains the predominance over that of expansion, and an interval of outward tranquillity ensues. During this the subterranean mass of lava which has been cooled down by the sudden partial vaporization of its aqueous particles, has its temperature again raised by the communication of caloric from below, till a second eruption is the consequence of the yielding of the congealed crust to its increasing force of expansion.

Rare instances of permanent eruption (as at Stromboli) are attributed to peculiar circumstances in the external figure and position of the volcano, owing to which the erupted matters escape from the orifice exactly in the ratio of the intumescence that takes place below from the constant accession of caloric; the repressive forces being in this case preserved in equilibrio with those of expansion. Such a volcano is of course as sensible as the barometer to all changes in the pressure of the atmosphere, and this in fact appears to be the case with the phænomena of Stromboli.

The author examines in detail, and by the strict rules of physical science, the laws which must regulate all the variations of the eruptive force, the duration of the quiescent intervals of an habitual vent, the quantity of gaseous liquid and solid matters produced, the modes in which they dispose themselves, the cooling and consolidation of lavas, their internal configuration, texture, and mineral characters, their subsequent alteration by acid vapours, or other agents, &c. and accompanies this investigation with corresponding facts from his own observation on the recorded testimony of others. Many of these are new, and highly interesting. Thus the great bulk of *Trachytic* when compared with basaltic lavas, is proved to be owing to their inferior specific gravity; examples are adduced of *trachyte* alternating in beds or currents with *basalt;* volcanic mountains are shown to be subjected to a process by which their great craters are alternately hollowed out by *paroxysmal* eruptions, and by degrees filled again to overflowing by the products of less violent explosions; the origin of trass, moya, and some tufas, is found in the bursting of lakes formed within the craters of *trachytic* volcanos; and it is shown that eruptions may be continually taking place from numerous vents at the bottom of the ocean without occasioning any visible commotion on its surface, until the summit of the submarine volcanic mountain is raised to within a short distance of that level, &c.

Every volcano is thus either a permanent or intermittent issue, by which the caloric of the interior of the globe passes off into outward space. By the more or less complete obstruction of these vents, it is forced to accumulate on different points beneath the superficial strata of the globe, until, by dilating the rocks in which it resides, it produces fractures sufficiently deep and large for its partial escape. In this manner the draught of caloric is often forced to shift its direction, and is diverted from one vent to another, the former becoming apparently extinct. In fact, it is observed that an eruption is seldom repeated a second time from precisely the same fissure; and in the same manner as a narrow cleft is sealed up by the consolidation of the vein of lava it contains, or the accumulation of loose fragments above it, and the local expansive force of the heated lava below, checked for

a time, and forced at length to break out by another issue; so the accumulation of similar obstructions, of greater magnitude, and during a longer period of eruption, checks the *general* expansive force of the subterranean caloric for intervals of great duration, and drives it to force an issue at length in some other direction, perhaps at a superficial distance of some hundreds of miles, leaving the former vent to all appearance extinct.

The jarring and vibratory motion occasioned by the sudden rupture of the overlying mass, is supposed to occasion the phænomena of earthquakes, which are felt over a greater or less surface according as the seat of expansion is confined to the focus of an habitual volcano, or exists at a greater depth, and extends more widely.

To repeated expansions of this nature taking place at a great depth, Mr. Poulett Scrope attributes the elevation of mountain chains; the fractures, contortions, and other irregularities of their strata resulting from the immense friction occasioned by the protrusion through them of the crystalline rocks in a solid state (granites, porphyries, serpentines, &c.), and the subsidences produced by their elevation to high angles while often in a semi-solid state.

But as it is not on these lines of greatest elevation that *volcanos* have usually burst forth, the author conceives this process to occasion the formation of parallel fissures at a distance, by the lateral *drag* accompanying the forcible elevation of the superficial strata; and that the sudden reduction of pressure on the internal mass of heated rock immediately under these clefts, or the largest and deepest of them, causes it to intumesce and break out on various points; thus giving rise to those *linear chains* of volcanic vents which are so remarkably obvious on the globe's surface, and which frequently present a decided parallelism to one another, and to some neighbouring range of continental mountains. The activity of these lateral vents proportionately retards the increase of expansive force general to the subterranean rock, and tends to check the farther elevation of the neighbouring continent. Hence volcanos act as *safety-valves* to the globe; while in their origin they are only secondary and attendant circumstances to the more immediate and primary result (as well as *final* cause) of subterranean expansion, viz. partial elevations of the solid crust of our planet.

This theory is then applied to account for the most remarkable features in the surfaces of continents; and the author goes on to examine, though in a more cursory way, as not properly belonging to his subject, the probable origin of the various stratified rocks, and the causes of their distinguishing characters.

This leads him to hazard a sketch of a theory of the globe,

which he conceives to have been at an intense temperature throughout, when first it reached its actual orbit, perhaps launched from the sun, according to Buffon's notion. The rapid and prodigious superficial vaporization to which it must have been subjected in open space, suddenly congealed a solid crust around it (granitic). On this the primæval ocean was deposited by the gradual condensation of the aqueous vapours which the force of gravity confined to the vicinity. The other aeriform fluids generated during its vaporization united to form the atmosphere of the planet. But as the caloric of the nucleus began again to spread towards the surface, expansions took place beneath this crust, which was broken through and raised on numerous points. In this age of turbulence the schistose rocks, gneiss, mica slate, and the earlier (so called) transition strata were deposited, the transported and suspended fragmentary particles (particularly the mica) subsiding to the bottom wherever the ocean was sufficiently tranquil, and the substances held in solution by it (carbonate and sulphate of lime, muriate of soda, &c.) being precipitated at the same time as its waters cooled. The mineral characters and organic remains of these rocks testify to these circumstances of the archaic ocean, as the inferior disturbance, the less crystalline texture, and more perfect organized remains of the later strata, indicate their formation at a period of less turbulence, when elevations of the globe's crust were less frequent and violent, when the temperature of the ocean and atmosphere had diminished, and the quantity of water taken into circulation through the atmosphere, and returned in torrents on the surfaces of the new continents, had proportionately decreased.

The author, after remarking on the wonder-working spirit which has dictated most theories on this subject, closes with these words: "The theory of the globe, which I have hazarded above, and which I am aware requires much ulterior developement, and perhaps some corrections, to render it complete, consists simply in the application of those modes of operation which nature still employs on a large scale, in the production before our eyes of fresh mineral masses on the surface of the earth, to explain the origin of those which we find there already. If, after fair discussion, and with all reasonable allowances, it is found adequate to this purpose, its truth will be established on the soundest possible basis—the same upon which rests the whole fabric of our knowledge on every subject whatsoever; the supposition, namely, that the laws of nature *do not vary*, but that similar results always are, have been, and will be produced by similar preceding circumstances."

An appendix is added, containing a list of known volcanos in recent or habitual activity; and an examination into the appa-

rently anomalous phænomena described by M. de Humboldt, as having accompanied the eruption of Jorullo, in Mexico.

The work is illustrated by an engraving, some lithographic drawings and maps, and numerous wood-cuts, introduced into the text.

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Fellows of the Royal Society held their anniversary meeting, as usual, on St. Andrew's Day, when the President, Sir Humphry Davy, delivered an eloquent address to the members present, on announcing the decision of the Council with respect to the award of the Copley medals. The medal of this year's donation, he informed them, has been bestowed on M. Arago, FRS. and Member of the Royal Academy of Sciences of Paris; and another, which was not disposed of on a former year, to Mr. Peter Barlow, FRS. and Professor in the Royal Military Academy at Woolwich.

"The discoveries and labours," said Sir H. Davy, "which your Council have made it their pleasure, and thought it their duty to honour, by conferring on their authors the highest rewards of this Society, both belong to the same department of science, *Magnetism*—a department which has always claimed a considerable portion of your attention, both in its relations to philosophy and utility, to the laws and properties of natural forms, and to navigation, the great source of the power and prosperity of this mighty empire."

Sir Humphry then entered into some historical details and general views on the subject of magnetism, in order more distinctly to state the grounds of the decisions of the Council, which he had just announced, and set forth the justice of the awards; and to prove, that "though much has been done, more still remains to be effected for the distinct knowledge of the laws and relations of these mysterious phænomena."

The property of the magnet to attract iron seems to have been known from the most remote antiquity; but its directive force, and consequently its use in navigation, was wholly unknown to the ancients. The period when the polarity of the magnet was first applied to maritime purposes, was some time between the years 1100 and 1300. The variation of the compass was discovered somewhere about the end of the 15th century, "probably in the two great voyages made to the eastern and western worlds, by Vasco di Gama, and Christopher Columbus." The dip of the needle was ascertained by Robert Norman, in London, in 1581,

and the change of variation was accurately demonstrated by Prof. Gellibrand, of Gresham College, in 1635.

In 1600, Dr. Gilbert, of Colchester, published, in Latin, his Treatise *de Magnete.* "In this truly philosophical and original work, the author endeavours to prove that the phænomena of magnetism are owing to the magnetic polarity of the earth,—that soft iron becomes a temporary magnet by the influence of the earth,—that in steel the magnetic property is induced by the same cause, with more difficulty indeed, but the effect is more permanent; and he explains the motion of the needle, and the powers of common magnets, by showing that opposite poles of different magnets attract each other in some definite ratio of their distance. He indulges, which could hardly be avoided in that age, in some vague hypotheses, and details some futile experiments; but notwithstanding this, his views display very extraordinary powers of mind; and though censured by his contemporary, Lord Bacon, for endeavouring to solve the phænomena of gravitation by magnetic attraction, yet his researches have a character of inductive reasoning perfectly in the spirit of the philosophy of that great man, who, had he studied his work with more attention, would have found in it numerous examples of his own sublime method of pursuing science, a contempt for the speculative authority of the ancients, and an appeal, almost new in those times, to the laborious method of repeated experiments."

The President then went on to mention the discovery of the diurnal variation of the needle in 1722 by George Graham; the labours of Lambert, Coulomb, and Robison on the laws that regulate magnetic attractions and repulsions, and the mathematical views of the theory of magnetism brought forward by Epinus and Robison on the hypothesis of a single magnetic fluid, and the recent memoirs of M. Poisson on the supposition of two fluids, an Austral and Boreal, presented to the Royal Academy of Sciences of Paris.

"The hypothesis of magnetic, which so closely agrees with that of electric fluids, has been defended by similar arguments, and illustrated by analogous experiments; and the connexion between these two classes of phænomena had been often observed and dwelt upon by philosophers." Beccaria supposed the magnetism of the earth to be produced by electric currents, and similar opinions were advanced and supported by vague analogies, and insufficient facts; but "till the important discovery of M. Œrsted, the true relations of magnetism and electricity were unknown." "I could with pleasure," said the President, "dwell on this discovery, and its immediate consequences in the developement of new and extraordinary results; and would the time allotted to a discourse of this nature allow it, I should have great satisfaction in describing to you the

labours and the discoveries of various philosophers belonging to this and the other learned Societies of Europe, and which have established, within the last five years, a perfectly new order of facts, not less brilliant from their striking and unexpected results, than important in their relations and theoretical applications to other phænomena of nature."

"I cannot, however, quit this part of my subject without calling your attention to the manner in which these discoveries have originated and been pursued, as it affords the most remarkable instance upon record of the unity of the laws of nature—of the mode in which remote phænomena are connected together, and the happy consequence of close attention to unexpected or uncommon results."

"A fact discovered by Galvani, and by him believed to be strictly physiological, investigated by the genius of Volta, was the origin of his wonderful pile, or battery; and this instrument, after its powers had been apparently exhausted in demonstrating new laws in electricity, and affording us new creations in chemistry—altering our arrangements and systems,—became, in the hands of the Danish philosopher, a source of novel and perfectly unexpected combinations, throwing a light upon parts of the corpuscular philosophy which were before in absolute darkness."

"Though the labours of M. Arago, which have been the object of the vote of your Council, cannot be considered as immediate consequences of M. Œrsted's discovery, it is probable they never would have been undertaken had not this discovery immediately excited the attention of their excellent author, who was one of the first philosophers that endeavoured to investigate, confirm, and illustrate the facts of electro-magnetism." Sir Humphry Davy next alluded to the idea of Coulomb (who was probably deceived by the presence of minute portions of iron in the substances he operated on) that all bodies in nature are capable of magnetic attractions; and the various experiments which were made, after the discovery that magnetism is a consequence of electrical action, to produce magnetic effects in other metals besides those long known to be capable of them, the results of which proved that the effects were transient, and disappeared with the electrical cause.

"Till M. Arago's inquiries, iron, nickel, cobalt, and their combinations, were the only species of matter apparently affected by magnets. His experiments extend this property under certain modifications to all metallic substances; and it is said, though as yet we have no distinct details, to various other bodies."

"M. Arago found that the extent of the vibrations of a magnetized needle was greatly diminished by holding over it a plate of copper; and by causing a plate of copper to revolve below it,

the direction of the needle was soon changed, it began to turn round, and the velocity of its revolutions increased, till they became too rapid to be counted.

"M. Arago made the same trials with other metallic substances, and with similar results, differing, as might be expected, in intensity;" and these experiments have been repeated, confirmed, and extended, by MM. Herschel, Babbage, and Christie.

"It is for the discovery of this fact, the power of various bodies, principally metallic, to receive magnetic impressions in the same, though in a more evanescent manner than malleable iron, and in an infinitely less intense degree, that your Council have awarded your medal to M. Arago; and you, I am sure, cannot but approve of their decision; for whether in its immediate relations, or ultimate applications, there is no physical fact which has been made known during the present year, that can with propriety be put in competition with it.

"By extending the empire of magnetism to a number of bodies, it removes much of what was mysterious and inexplicable in that department of science, and renders it a branch of the general philosophy of nature; and when the new analogies between magnetic and electrical action established by these phænomena are considered, there is much reason to hope that they may be ultimately referred to the same cause, and, with chemical affinity, possibly be found identical with the general quality or power of attraction of gravitation."

Sir Humphry Davy then adverted to the scientific labours of Mr. Barlow, and passed a well-merited encomium on his papers published in the Transactions of the Royal Society, which, he observed, establish his character as a judicious and accurate experimenter, and an able reasoner; but though the facts and reasoning brought forward in those memoirs would undoubtedly have claimed the attention of the Council, and might have led them to balance Mr. Barlow's merits with those of other contributors to the Philosophical Transactions, "their opinion was fixed, and their decision formed, by a practical application of science of great ingenuity and of considerable utility."

Masses of iron become magnetic by the action of the earth: a bar of soft iron, for instance, held vertically, has its north pole uppermost, and attracts the needle in the same manner as the pole of the earth; and all masses of iron, following the same law, exert an action on the needle proportional to the square of the distance, and of course destroy, or diminish in a certain ratio, the action of the north pole of the earth. "It is extraordinary that so important a circumstance as the action of the iron in a ship on the needle had not earlier and more strongly arrested the attention of navigators. Even Dr. Halley, the most accomplished and profound philosopher that ever made long

voyages, though he observed the effect, does not seem to have thought it worthy of correction, and that even when making a set of minute observations on variation; he says, in his paper in the Transactions, ' We know by experience how little the iron guns on board ship affect the needle.' This, however, probably arose from the circumstance, that he was never in very high latitudes." Walker, in his Lecture on Magnetism, first called the attention of navigators to the subject; Capt. Flinders brought it before the notice of the Admiralty; and the late arctic expeditions have afforded the fairest and fullest opportunities of determining the fact.

" Mr. Barlow, after making a number of experiments on the phænomena presented by different large masses of iron, and recurring to the principle that the contiguity of a small mass makes it equal or superior in power to larger masses; and that the attractions and repulsions diminish as the square of the distance, thought of two methods of correcting the errors arising from the magnetism of the iron in ships; one by compensating, the other by doubling them, by means of small masses, or thin plates of iron placed near the compass, the relation of which to the magnetism of the earth, the iron in the ship, and the needle, should be determined by experiments."

Mr. Barlow has adopted the last method in practice, and its utility has already been proved by Captains Baldey, Sabine, and Parry, and other able and enlightened officers.

" The Royal Society " the President continued, "has always since its first institution, given particular encouragement, and paid particular attention to those departments of science which are strictly practical, and offer the best vindication and the highest praise of the experimental and inductive method, bringing philosophy, as it were, from the heavens to the earth, and fixing her abode, not in visionary, splendid, and airy edifices, but amongst the resting places and habitations of men. To point out an useful application of any doctrine or discovery has always been their highest pride, and fortunately they have had many noble opportunities and examples. Indeed there is scarcely any instance of a considerable advance made in the knowledge of nature without being soon connected with some tangible benefit or advantage; as light is almost always accompanied by heat, the illuminating by the productive and nourishing principle.

" In conformity to the usages and feelings of the Society, the Council has awarded the medal to Mr. Barlow, who, by reasoning and experimenting upon a few simple facts long known, but never applied, has founded an useful invention, tending to the perfection of an instrument, the most important, perhaps, to Britons, of all those which have been the results of scientific

principles; increasing the perfection of an art which is not only one of the greatest sources of our power, but a bond of union amongst nations, securing their intercourse, and extending the progress of commerce, civilization, and refinement."

Sir Humphry Davy then turned to Mr. South, who had undertaken to forward the medal to M. Arago, and addressed him in the following words:—"Mr. South: In transmitting this medal to M. Arago, assure him of the interest we take in his ingenious and important researches, and inform him that we wait with impatience for the continuation of his labours on this new and fertile subject. As one of our Fellows, his discoveries have the same interest for us as they have for his brethren of the Royal Academy of Sciences, which, for more than a century and a half, has gone on encouraging and emulating our labours. You and our worthy Secretary * are an example of recent liberality on their part, and of the respect paid to British talent. We, I trust, shall never be behind them in dignity and nobleness of sentiment;—far be from us that narrow policy which would contract the minds of individuals, and injure the interests of nations by cold and exclusive selfishness,—which would raise the greatness of one people by lowering the standard of that of another. As in commerce, so in science, no one country can become worthily pre-eminent, except in profiting by the wants, resources, and wealth of its neighbours. Every new discovery may be considered as a new species of manufacture, awaking novel industry and sagacity, and employing new capital of mind. When Newton developed the system of the universe, and established his own glory and that of his country on imperishable foundations, he might be regarded as giving a boon to the civilized world, for which no adequate compensation could ever be made; yet, even in this, the most difficult and sublime field of discovery, Britain has been repaid, if not *fully* yet *fairly*, by the labours of Euler, La Grange, and, above all, La Place, perfecting the theory of the lunar motions and planetary perturbations, and affording data of infinite importance in the theory and practice of navigation. Science, like that nature to which it belongs, is neither limited by time nor space; it belongs to the world, and is of no country, and of no age; the more we know, the more we feel our ignorance, and how much still remains unknown; and in philosophy the sentiment of the Macedonian hero can never apply; there are always new worlds to conquer."

To Mr. Barlow the President spoke as follows:—"Mr. Barlow: I have great pleasure in presenting you with this medal in the name of the Royal Society. Receive it as the highest mark of distinction which they have the power to bestow. You have

* Mr. Herschel.

already been honoured by marks of approbation both at home and abroad, far more valuable in a pecuniary point of view, but no one which, I think, ought to give you more *durable satisfaction*; for this award has, I believe, never been made except after dispassionate and candid discussion; never to gratify private feelings, or to call for popular applause; and amongst those philosophers who have received it are names of the very highest rank in science. We trust, both on account of the public good and your own glory, that you will engage in and accomplish many new labours. You have not merely had *scientific* success, but one still more gratifying to your heart and feelings—the idea that you have been useful to your country, and secured the gratitude of a body of men who are not tardy in acknowledging benefits."

The Society then proceeded to the election of Officers and Council for the ensuing year. The following were chosen:—

Of the Old Council.—Sir H. Davy, Bart.; Francis Baily, Esq.; W. T. Brande, Esq.; Samuel Goodenough, Lord Bishop of Carlisle; Davies Gilbert, Esq.; J. F. W. Herschel, Esq.; Sir Everard Home, Bart.; Capt. H. Kater; John Pond, Esq.; W. H. Wollaston, MD.; Thomas Young, MD.

Of the New Council.—John Barrow, Esq.; John Bostock, MD.; Sir A. P. Cooper, Bart.; Benjamin Gompertz, Esq.; Stephen Groombridge, Esq.; Sir Abraham Hume, Bart.; Daniel Moore, Esq.; Richard, Earl of Mount Edgecombe; P. M. Roget, MD.; James South, Esq.

President.—Sir H. Davy, Bart.

Secretaries.—W. T. Brande, Esq. and J. F. W. Herschel, Esq.

Treasurer.—Davies Gilbert, Esq.

Dec. 8.—A paper was read, entitled "Additional Proofs of the Source of Animal Heat being in the Nerves. By Sir E. Home, Bart. VPRS."

Dec. 15.—The President announced to the Society His Majesty's munificent foundation of two annual prizes, consisting each of a medal of the value of fifty guineas, to be bestowed as honorary distinctions, by the President and Council, on the authors of such new discoveries as they may deem worthy of the award; and in such manner as shall best promote the objects for which the Royal Society was instituted, and the interests of science in general.

Dr. J. R. Johnson, elected into the Society in 1817, and whose name had been then inserted in its printed lists, was admitted a Fellow of the Society; and the Croonian Lecture, by Sir E. Home, was read. The subject of this Lecture was the structure of muscular fibre.

Dec. 22.—Gideon Mantell, Esq. was admitted a Fellow of the Society; and the following papers were read, of which we shall give some account in our next:—On the Poison of the

Common Toad; and, On the Heart of Animals belonging to the Genus Rana; both by J. Davy, MD. FRS.

The Society then adjourned over the Christmas vacation.

LINNEAN SOCIETY.

Dec. 6.—A continuation was read of A Systematic Catalogue of the Australian Birds in the Collection of the Linnean Society; by N. A. Vigors, Esq. FLS. and T. Horsfield, MD. FLS.

Dec. 20.—The reading of the Catalogue of Australian Birds was continued; and a paper was also read, containing Descriptions of some new Species of Birds belonging to the genera *Phytotoma, Indicator,* and *Cursorius;* by Mr. Benjamin Leadbeater, FLS.

ASTRONOMICAL SOCIETY.

Dec. 9.—The President informed the Society, that when he had the honour of announcing at their last meeting, the extraordinary occurrence of the appearance of *four* comets in the short space of as many months, he was little aware that he might at that time have added a *fifth* to the number. This last comet appeared, from the account stated in the public journals, to have been discovered by M. Pons, at the beginning of last month; but, as it had considerable *south* declination, and was advancing also to the southward, and at the same time very faint, it probably would not be seen in this country.

Although the appearance of so many comets in one year had been mentioned as a remarkable phænomenon, yet he would not wish to be understood as supposing that such a circumstance had never previously occurred, nor was likely to occur again. The fact was, that from the great attention which had been paid by astronomers to the discovery of these bodies within these few years, and the interest excited by the investigation of the laws by which they were governed, a more than ordinary diligence had been employed in searching for them. And there was every reason to believe that if there were more labourers in the field, a still richer harvest would ensue: from which there might fairly be expected some additional light on the laws and constitution of the universe.

The President likewise called the attention of the members to the circumstance of the opposition of Mars in the month of May in the ensuing year. It was well known, he remarked, that by a comparison of the observations of this planet with the stars which were near it at that time, made at places situated in these latitudes and at other latitudes having considerable southern declination, the parallax of the planet might be readily deduced, and thence the parallax of the sun. As there were, at this time, two active observatories in the southern hemisphere, where this phænomenon would probably be attended to (as it had been at the two preceding oppositions), it were extremely desirable that

corresponding observations should be made in the northern hemisphere; without which the observations made in the south would (as far as this subject is concerned) be rendered of little or no use. He trusted therefore that those practical astronomers who were possessed of the requisite instruments (and they were by no means complex or expensive) would attend to this phænomenon, and record the observations which they might have the advantage and opportunity of making; the uncertainty of this climate rendering it extremely desirable that all those who had the means should unite in so useful an undertaking.

For the convenience of such observers, the President announced that he had computed the right ascension and declination of six stars, near which Mars would pass a few days before and after his opposition: these being the whole which he could find in any of the catalogues. They were here offered only as a mean of identifying the star with which the planet may be compared. It was probable that other stars might be seen, in the field of view of the telescope; and that even some of these might not be found; for the catalogues of the smaller stars are still very imperfect. It would render observations of this kind more complete and useful, if regular observations of such stars as might be situated near Mars at the time of his opposition, were made at the public observatories; whereby the true position of the planet in the heavens would be more correctly ascertained.

The following are the mean positions of the stars above alluded to on the 1st of January, 1826:—

Star.	Mag.	AR	D.
8 Libræ	6	$14^h 41^m 5^s$	−15° 16′ 2″
α ——	3	41 16	15 18 42
(195) P	8·9	43 0	15 40 36
L. L. X.	7	46 38	16 5 12
(252) P	8·9	53 51	15 54 8
υ^2 ——	6·7	57 7	15 48 13

The reading of the description of the large reflecting telescope and frame made by Mr. John Ramage, of Aberdeen, was terminated. Mr. Ramage has, ever since the year 1806, devoted much of his time to the construction of reflecting telescopes of large size, and of convenient frames and supports, in which firmness of structure and facility of adjustment to any required position, should be equally attained. The telescope now described has a twenty-five feet tube. The platform upon which the telescope is placed, and revolves at pleasure, is a strong circular rail-way of cast iron, twenty-seven feet and a half in diameter, and four inches in breadth. The horizontal azimu-

thal motion is upon concentric rollers, round a central pivot. The stand or frame, though simple in its construction, cannot be very intelligibly described without a model or a diagram. The tube of the telescope is elevated to the required altitude by a winch and tackle of pulleys. The gallery in which the observer stands is adapted to the proper height by a similar winch and tackle; and to prevent accident from the breaking of the ropes, it is supported at each side by two moveable bars that fall into the steps of the ladders, which constitute a part of the frame. The lower end of the tube rests upon two rollers, and at great altitudes moves forwards, so that the tube itself is capable of adjustment to all positions, from that, which is nearly horizontal to that, which is nearly vertical. Without quitting the gallery, the observer can move the tube both horizontally and vertically upwards of 10°, and can with the utmost readiness (independently of an assistant) direct the telescope to any point in the heavens. All the motions are effected by means of a very few cords, pulleys, and winches. The diameter of the speculum is fifteen inches, and the focal length twenty-five feet. The eye-pieces, which are adapted to magnify the image, possess powers varying from 100 to 1500; and there are proper diaphragms to modify the redundancy of light. The mode of observing is by the "front view."

Mr. Ramage exhibited to the Society, besides a neat model of the tube and apparatus, two speculums; one of fifteen inches diameter, belonging to the telescope described, and another of twenty-one inches diameter and fifty-four feet focus.

There was next read a paper on the subject of Parallaxes, taking the word in an enlarged sense, by M. Littrow. It was in the excellent treatise of Lagrange on the determination of the solar parallax, from the observed transits of the inferior planets over the sun's disc, where the rectangular co-ordinates were first employed, instead of the less convenient expressions of spherical trigonometry, for the purpose of deducing the apparent station of a planet from its longitude and latitude. The process has been since improved by Olbers, Bessel, Rhode, &c. But M. Littrow regards it as susceptible of still further improvements, which he has here exhibited. He gives the analytical solution of several problems; viz.

1. To determine the apparent longitude and latitude of a star, from the true geocentric longitude and latitude.

2. To solve the inverse problem.

3 and 4. The solution of the preceding problems by series.

5. To find the apparent right ascension and declination, from their true magnitudes, and *vice versâ*.

6. To determine the apparent azimuth and altitude, from their true magnitudes, and *vice versâ*.

7 and 8. To find the true place of the star, from its apparent

place, and *vice versâ,* without any reference to the horizon, the ecliptic, and the equator, which is often useful in computing the occultation of fixed stars by the moon.

9. A general problem, to find the apparent azimuth and apparent altitude, from the true longitude and the true latitude of a star.

The resulting expressions for these several solutions are analytically simple. Those which are deduced in series are usually of this kind, namely,

$$\log. c = \log. b - \left(\frac{a}{b}\right) \cos. \theta - \tfrac{1}{2}\left(\frac{a}{b}\right)^2 \cos. 2\,\theta$$
$$-\tfrac{1}{3}\left(\frac{a}{b}\right)^3 \cos. 3\,\theta, \text{ \&c.}$$

in which the *law* is evident.

M. Littrow concludes his paper by suggesting the application of his principal formulæ to the solution of various other problems.

Lastly, there was read a paper, entitled "A Memoir on different Points relating to the Theory of the Perturbations of the Planets expounded in the *Mécanique Céleste;*" by M. Plana, Astronomer Royal at Turin, and an Associate of this Society.

The object of the author in this memoir he states to be an examination of various points in the theory of the planetary perturbations as explained by M. de Laplace in the *Mécanique Céleste.* In undertaking this labour, he observes, he at first had no expectation of meeting with any instance in which an actual rectification of the results already arrived at would be necessary; but the progress of late made in the theory of perturbations having enabled him to treat certain particular questions more generally, and with more symmetry than heretofore, it is not to be wondered at if he has been led to results which surpass in exactness those hitherto published. But in all such cases, he adds, where he has arrived at conclusions not in accordance with those of the illustrious author of the *Mécanique Céleste,* he has thought it incumbent on him to give with the fullest detail, not only the developments, but even the arithmetical calculations on which these conclusions have been founded.

The first chapter is devoted to the consideration of that artifice in the *Mécanique Céleste* in which M. Laplace transfers his formulæ from the mean motions, axes, &c. of the primitive or undisturbed orbits, which are not given by observation, to those of the disturbed, which are given as they exist in nature. This he does by assuming an arbitrary constant introduced in one of the integrations by which the perturbation in longitude is derived, in such a manner as to make the term in the result which depends on the mean motion vanish. M. Plana devotes this chapter to the elucidation of this artifice, and shows the correctness of M. Laplace's results by obtaining the same con-

clusion by another and direct method. He then applies his reasoning to numerical examples, and computes the quantity by which the moon's mean distance from the earth is *permanently* altered by the sun's action, which he finds to be about 1-100th of the radius of the globe of the moon, in augmentation, the corresponding increase of the periodic time being about one-fourth of a day. The excentricity too undergoes an alteration in its *mean* quantity from the same cause, equal to about 0·0007 of its actual amount.

A similar artifice in the use of an arbitrary constant added in one of the necessary integrations for arriving at the first term of the motion of the moon's perigee, M. Plana observes, has enabled M. Laplace to avoid an error in that research to which his method seemed to expose him, and to obtain the true result. But he proceeds to show that this artifice is not necessary, and that the same result may be obtained without the use of the superfluous constant, by the aid of an equation he deduces for the variable portion of the moon's radius vector.

The method employed by M. Plana has the advantage, he observes, of keeping distinctly in view throughout the whole analysis the primitive elements, uninfluenced by the effect of perturbation. The other he states to have been first employed by Lagrange in the volume of the Memoirs of the Academy of Berlin for 1783.

The author next proceeds to examine those parts of the theory of perturbations, which depend on the non-sphericity of the central body, and in which he remarks that the use of a similar artifice in the *Mécanique Céleste* is accompanied with greater obscurity, as a portion only of the arbitrary constant is retained. He therefore enters on the investigation without the use of this artifice, and deduces the results for the perturbations of the planets due to the ellipticity of the sun by the formulæ for the variation of the arbitrary constants.

The author next applies the same method to the theory of the perturbations of the seventh satellite of Saturn by the elliptic figure of the planet; and as he here arrives at final equations somewhat differing from those of M. Laplace, the whole process is given in copious detail.

The second chapter of this paper is devoted to the consideration of the effect of the actions of the fixed stars on the secular variations of the planetary system. The expressions for the secular variations of the excentricity and aphelion which the author brings out, agree perfectly with Laplace's in form, but differ in the numerical coefficients, one of the terms having the coefficient $\frac{15}{4}$ where Laplace has $\frac{3}{4}$, and another $-\frac{3}{2}$, where Laplace makes it -1. As he subsequently observes, however, the action of the stars cannot possibly become sensible till after the lapse of many hundreds of centuries; so that these discre-

pancies are practically of no importance. He remarks too that this cause of perturbation prevents the equations between the squares of the excentricities, the masses, and square roots of the axes, so often referred to as insuring the stability of the planetary system,—as well as the similar one between the squares of the tangents of the inclinations, the masses, and square roots of the axes,—from being mathematically exact. It will be noted, however, that these equations can only be regarded as proved for the first powers of the disturbing forces, while the action of the stars is at least of the order of their squares or even cubes.

The third chapter is devoted to the evaluation of those terms in the theory of the perturbations of Mercury by the Earth whose coefficient, being divided by the square of the difference between the mean motion of Mercury and four times that of the Earth, may acquire a notable value by the smallness of its divisor. The author first examines the indirect method followed by M. Laplace, which he considers defective and in some measure illusory, and then substitutes a method of his own. After going through all the very laborious calculations of the analytical and numerical values of the coefficients he arrives at a final result, of which he remarks that although it differs very little from that given in p. 98 of the third volume of the *Mécanique Céleste*, and in p. 32 of the tables of Mercury published by M. Lindenau, yet this apparent accordance is merely a consequence of the excessive smallness of the numerical coefficient of the term in question, and that his object has rather been to rectify the analytical formulæ than the numerical results, by taking into consideration *all* the terms of the same order; without which he considers it very possible to commit material errors in the final results of such operations.

The fourth chapter has for its object an examination of M. Laplace's method of taking account of the square of the disturbing force in the theory of the great inequality of Jupiter and Saturn.

In this investigation the author is led to conclude, that the equation connecting the reciprocal perturbations of the mean motions of two planets, and by which the one may be derived from the other by a simple multiplication, holds good only when the first powers of the disturbing forces are considered (a consequence, it may be observed, one might naturally presume from the form of the multiplier itself, into which the simple ratio of the masses only enters as a factor).

$$\left[\zeta' = -\frac{m}{m'}\sqrt{\frac{a}{a'}}\,\zeta,\right].$$

M. Plana gives this part of his paper with the fullest possible detail, in order, he observes, to enable astronomers to verify

every part of the developments and calculations; and on reducing his formulæ to numbers, obtains (not, as he says, without surprise) a final result, of a contrary sign to that of Laplace, and only one-third of its amount, the coefficients of the terms of the great inequality arising from the square of the disturbing force being, according to M. Plana,

$$-\ 1''{\cdot}9200 \text{ and } +\ 5''{\cdot}5775 \text{ for Jupiter}$$
$$+25''{\cdot}1036 \text{ and } -12''{\cdot}8932 \text{ for Saturn.}$$

The fifth chapter contains reflections on the Supplement to the theory of Jupiter and Saturn in the fourth volume of the *Mécanique Céleste* (p. 327—344); in which M. Laplace considers several terms of the order of the square of the disturbing force arising from the variation of the excentricities and perihelia of the two planets, affected by the argument of the great inequality. M. Laplace has made use of an indirect but more expeditious method; and the object of the author in this chapter (admitting, however, that the indirect method cannot fail to give results very near the truth) is to estimate their degree of accordance with those afforded by the direct method. His conclusions in a numerical point of view agree with those of Laplace, but he conceives that his analysis is more rigorous, and his formulæ better adapted to further developments.

Article XII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Notice of Dr. Thomson's New Work.*

After the severe animadversions on Dr. Thomson's First Principles of Chemistry, which appeared lately in a cotemporary journal, it is but fair that our readers should know in what estimation that work is held by our philosophical brethren of the New World. We, therefore, extract the following from the first number of the 10th volume of the "American Journal of Science and Arts," which has this moment reached us:—

"It is not within our purpose, or present limits, to do any thing more than briefly mention the admirable work of Dr. Thomson. For more than twenty years, we have diligently followed this distinguished author through all the numerous editions of his Systematic Chemistry, in which he has shown himself the vigilant and faithful historian of the science; through his *Annals of Philosophy*, one of the best of the numerous

scientific journals of this day, and through his smaller works, and separate memoirs, with which he has favoured the public; and we have listened with high interest to the instructions of his lecture room. But after all, predisposed as we were to expect much from a great effort of the mature age of such a master, our expectations have been more than equalled. There is nothing, the offspring of the present age, which, so far as we are informed, surpasses this 'Attempt to establish the First Principles of Chemistry by Experiment.' The vast amount of labour performed,—the patient and persevering repetition of tedious and often difficult processes, frequently to the eighth or tenth time—the consummate skill discovered in devising and executing the experiments, and the surprising coincidence of the results of analysis with the deductions of theory, excite our astonishment, and prove beyond a question, that chemistry, if not founded on intuitive, is built on demonstrative truth. Dr. Thomson, after performing so much, might well have adopted a motto preferring higher claims than that which he has chosen." —(Silliman's Journal.)

2. *Discovery of Lithia in the Mineral Waters of Bohemia.* By M. Berzelius.

An extract of a letter from M. Berzelius to M. Dulong was read to the Philomatic Society on the 27th of August, 1825, in which that philosopher states, that on a fresh examination of the mineral waters of Bohemia, he has discovered that lithia is one of their constant and essential elements, a substance hitherto only found in a few minerals. To detect and separate the lithia, M. Berzelius pours a solution of phosphate of soda into the mineral water, evaporates to dryness, and redissolves in cold water all that is soluble in that menstruum. If lithia be present, it is left in the state of an insoluble phosphate of lithia and soda. The illustrious author of this discovery considers it as very probable that lithia exists also in sea water. The Editor of the Bulletin des Sciences, from which we have extracted this notice, states in a note, that he finds in the *Alg. Letterbode* of Aug. 5, 1825, that M. Mulder announces that he has made some experiments on the water of the Zuiderzee, but has not discovered any lithia in it.

3. *Chemical Examination of Peridot.* By M. Laurent Pierre Walmstedt, Professor of Chemistry at Upsal.

M. Mitscherlich, in his memoir on the relation which exists between chemical proportions and crystalline forms, has stated the composition of Peridot, which he there designates as the only known compound of silica with isomorphous bases with two atoms of oxygen, in which the quantities of oxygen of the silica and base are equal. He considers peridot as a silicate of

magnesia, combined with a certain quantity of silicate of protoxide of iron. In fact the measurement of crystals of the artificial silicate of iron and that of peridot demonstrate a perfect identity.

M. Walmstedt's analysis of the olivine of Mount Somma fully confirms these views. It gives

		Oxygen.
Silica.	40·08	20·16
Magnesia	44·24	17·12
Protoxide of iron.	15·26	3·47
Protoxide of manganese	0·48	
Alumina	0·18	
	100·24	

And since the quantity of oxygen in the magnesia is equal to five times that of the protoxide of iron, and the oxygen of the silica equal to that of the two bases together, it follows that olivine consists of one atom of silicate of protoxide of iron, and five atoms of silicate of magnesia.—(Annales des Sciences Naturelles.)

MINERALOGY.

4. *Comparative Analysis of the Elastic Bitumen of England and France.* By M. Henry, the younger.

The French bitumen was found in October, 1816, by M. Ollivier, in the coal mines of Montrelais, distant a few leagues from Angers, at the depth of 35 fathoms, in a rock of ophiolite, mixed with veins of quartz and carbonate of lime.

Physical Characters of the two Bitumens.—1. That found in Derbyshire is in brown, or blackish masses, slightly translucent on the edges, and appears greenish by transmitted light; it is more or less soft and elastic; burns very readily with a white flame, and exhales a bituminous smell. Its specific gravity varies from 0·9053 to 1·233. 2. The French bitumen presents nearly similar characters; its colour is very deep blackish-brown; it is opaque, inodorous, moderately compact, compressible, very tenacious, and particularly very elastic; by transmitted light it is rather black than greenish. It floats on water, and burns with a clear bluish-white flame and a bituminous odour.

The composition of the two bitumens, according to M. Henry's analysis, is, per cent.

	English elastic bitumen.	French elastic bitumen.
Carbon	52·250	58·260
Hydrogen	7·496	4·890
Nitrogen	0·154	0·104
Oxygen	40·100	36·746
	100·000	100·000

The great quantity of oxygen found in these bitumens tends to confirm (says the reporter in the Bulletin des Sciences) Mr. Hatchett's opinion as to the formation of elastic bitumen, and, to a certain point, allows us to compare the action of air on the liquid bitumen, such as naphtha, petroleum, &c. with that of oxygen on the fixed and volatile oils, which harden by being long in contact with that agent.—(Bulletin des Sciences.)

Zoology.

5. *On the Species or Varieties of the Human Race.*

Linnæus, in his "Systema Naturæ," divided man into four varieties, according to the colour of the skin, giving each variety the name of the part of the world where it was most common. Dumérille considers that there were six distinct varieties, which he names, 1. Caucasian, or European Arabs; 2. Hyperborean; 3. Mongolian; 4. American; 5. Malay; 6. Ethiopian. Cuvier reduced the number of varieties to three. Virey, in his History of Man, divided the genus into two species, according to the facial angle, noting three varieties, and subvarieties to each species. Desmoulins has lately further divided the genus man into eleven species; and Bory Saint Vincent, in a very elaborate paper on the varieties and species of this genus, has added four other species to this extended list, and has given the peculiarities, habits, manners, and appearances of each of the species, and an account of their probable origin. He divided the genus into two sections: the first, he called *Leiotrichi*, or smooth-haired men, which he again subdivides into those which are peculiar to the Old World, as 1. *Homo Japeticus*, the sons of Noah, which he divided into several races; 2. *Homo Arabicus*, the Arabs; 3. *Homo Indicus*, the Hindoos; 4. *Homo Scythicus*, the Scythians; 5. *Homo Sinicus*, the Chinese. Secondly, those smooth-haired men which are eommon to the Old and New World, as, 6. *Homo Hyperboreus*, the Laplanders; 7. *Homo Neptunians*, the Malays and New Zealanders; 8. *Homo Australasicus*, the New Hollanders. Thirdly, the straight-haired men, which are peculiar to the New World, as, 9. *Homo Colombicus*, the Caraibes; 10. *Homo Americanus*, the Americans; and 11. *Homo Patagonicus*, the Patagonians. The second section he designates by the name of *Oulotrichi*, or crisped-haired men, which are usually called Negroes. The white varieties of this tribe are not known. 12. *Homo Œthiopicus*, the Œthiopian; 13. *Homo Cafer*, the Cafre; 14. *Homo Melaninus*, the Colchin Chinese; and 15. *Homo Hottentotus*, the Hottentots.

6. *On the Plumage of the Oyster Catcher.*

It is generally considered that the Oyster catcher, the *Hæmatopus Ostralogus, Lin.* regularly change their plumage at the commencement of winter. Mr. Boie has lately had reason to

doubt this opinion; for in the month of November, 1821, he observed a great number of these birds, some of which had white, and others black throats; and judging from the consistence of the beaks and bones of them, he is inclined to believe that the former were the young, and the latter the old specimen of this bird. He also observed the same difference in the months of January and February of the following year.

7. *On the Change in the Weight of Eggs during Incubation.*

MM. Prevost and Dumas have lately given a very extensive paper, with numerous experiments, on the loss of weight which takes place during incubation, and they come to the following conclusions:—

1. That fecundated and non-fecundated eggs suffer nearly the same loss in weight during the period of incubation.
2. That the loss follows in both cases a decreasing progression from the commencement of the incubation.
3. That a remarkable proportion is observed between the duration of the incubation and the daily loss of weight, which is so much the smaller as the period of incubation is the longer.
4. That the loss of weight appears to depend entirely on the evaporation, or rather on the chemical alterations which take place in the contents of the egg, independent of the evolution of the fœtus, inasmuch as it is proportional to the duration of the incubation, and no ways connected with the slower or more rapid developement of the young animal.—(Ann. des Sci. Nat.)

MISCELLANEOUS.

8. *Weights and Measures.*

Our readers are probably aware of the alteration that is about to take place in the standard for weights and measures, and the change is so considerable as to form a distinct era in the metrological annals of this country. We subjoin the most important facts connected with the subject.

All the powers and regulations contained in the various former acts, authorizing the searching for, seizing, and destroying of weights and measures not conformable to the standards, are, with the penalties and forfeitures, declared to apply to standards established by these acts.

The standard of weight is the pound troy of 5760 grains, and the avoirdupois pound is declared to contain 7000 of such grains. The legislature has therefore made no alteration in the weights; but an alteration, to the extent of about one grain in the avoirdupois pound, has become necessary, from some supposed inaccuracy in the old standards kept in the exchequer.

The measures for length and superficies are not altered.

The standard measures of capacity, as well for liquids as dry goods, are all founded on the imperial gallon, established by

these acts, and directed to contain 10 lbs. avoirdupois of distilled water, at 62° of heat, the barometer being at 30 inches; the same being in bulk equal to 277·274 cubic inches; and all other measures are to be taken in parts or multiples of the said gallon.

The quart to be one-fourth part of the gallon, and to contain 69·3185 cubic inches, or $2\frac{1}{2}$ lbs of water.

The pint to be one-eighth part of the gallon, and to contain 34·65925 cubic inches, or $1\frac{1}{4}$ lb. of water.

Two gallons to be a peck, and to contain 554·748 cubic inches, or 20 lbs. of water.

Eight gallons to be a bushel, and to contain 2218·192 cubic inches, or 80 lbs. of water.

Eight bushels to be a quarter of corn, or other dry goods.

No particular form or proportion is directed for the bushel or other measures used for corn or other goods, usually sold by strike measure. The strike is directed to be round and straight, and of the same diameter from end to end.

The standard for coals, lime, fruit, and other goods, commonly sold by heaped measure, is the aforesaid bushel, to be made round (we must presume cylindrical), with a plain and even bottom, and nineteen and a half inches diameter from outside to outside; and when used, to be heaped in the form of a cone; such cone to be of the height of at least six inches, the outside of the bushel being the extremity of the base of such cone; and including the cone, it will contain 2815.4887 cubic inches, which gives 597.2967 cubic inches, being 26 and 12-13ths per cent. or 2 and 2-13ths of a gallon above the corn bushel, for the contents of the cone.

All other measures used for coals and other goods, sold by heaped measure, are to be made cylindrical, with the diameter at the least double the depth thereof, and the height of the cone three-fourths the depth of the measure.

The proportion that the new imperial measure of capacity bears to the various old measures is as follows:

—1·9492, say a fifth part, or 20 per cent. larger than the wine measure.

1—59·67, say a sixtieth part, or $1\frac{2}{3}$ per cent. smaller than the beer measure, which difference is equal to little more than one cubic inch in a quart.

This difference from the old coal measure will be very trifling, it being only 3-5ths of a pint larger in a chaldron.

A further alteration is made, by inference, from the declared proportions of the weight and cubical contents of the imperial gallon, which is interesting to scientific men; viz. that the specific gravity of water, hitherto established at 1000 ounces per cubic foot, is now calculated at the rate of 997·1364 ounces per foot.—(Scientific Gazette.)

9. *Mode of securing Anatomical Preparations.*

Dr. Macartney, of the University of Dublin, has employed a thin plate of India rubber, as a covering for preparation jaws in place of the former laborious and offensive one by means of putrid bladder, sheet-lead, &c.

It is essential that the India rubber should be painted or varnished, after which not the slightest evaporation of the spirits can take place. The material by its elasticity adapts itself to the variations in the volume of the contents of the jaw from different temperatures, and thus removes the principal cause of the evaporation of the spirits.

It is probable that leather coated with Indian rubber and painted would answer as well as the rubber itself, by which the expence would be greatly diminished.

10. *Medical Matriculation at Edinburgh.*

From the following statement of the number of medical matriculations which have taken place at the University of Edinburgh during the last five years, it will be seen that the afflux of students to this school still continued to increase.

In 1820 to 1821	754
1821 to 1822	817
1822 to 1823	867
1823 to 1824	870
1824 to 1825	905 to April.

A considerable number are stated to have been in attendance on the medical classes when this return was made up who had not then matriculated, many of whom were, however, expected to do so in the course of the summer sessions.

11. *Proportion of Male and Female Children.*

M. Bailly, of the French Institute, has lately made a series of observations connected with the subject of the relative births of male and female children. From exact registers kept in one locality, it appears, he says, that there were more female conceptions than male conceptions in the months of March and July; and these two months, he observes, are, the first on account of the occurrence of heat; and the second, on account of the heat of the weather, the part of year least favourable to the activity of the generative powers, at least with a view to fecundations.

12. *Pouillet on Atmospheric Electricity.*

Various theories have been formed by meteorologists to account for the electricity sensibly present in the atmosphere. Of these, Volta's was perhaps the only plausible one. That philosopher was induced to believe, that bodies in passing

from one state to another undergo a change in their electric condition; and he supposed that the electricity lost in storms was continually being renewed by that produced by evaporation perpetually going on from the surface, as well of the land as of the water.

The recent and interesting researches of Pouillet were instituted not merely to ascertain the truth of the Italian professor's hypothesis, he was also desirous of discovering the efficiency of another cause, which he believed to be of no small importance in the production of electricity, and of bringing to proof a theory of his own relative to the distribution and accumulation of this principle in the atmosphere.

Numerous and varied experiments have brought him to the conclusion, that the mere passage of a body from the solid form to a state of vapour is unaccompanied by the development of electricity; that the result is similar when vapour is condensed either into the liquid or solid form.

He conceived that Volta, though too accurate an observer to be mistaken as to the fact of the presence of electricity in his experiments, was, nevertheless, deceived, as to the cause of its production, by the formation of carbonic acid, which mixed with the vapour of water and complicated his experiments.

In 1782 Volta, Lavoisier, and Laplace showed that electricity was developed during chemical action; but as experiments relating to this point are liable to afford different and contradictory results from slight differences of circumstances, the question has been regarded as rather undecided. It became, on this account, an object of special attention with M. Pouillet. He finds, that in the combustion of charcoal there is an unequivocal production of electricity—that the acid produced is in the positive state, while the charcoal always becomes negative. It is necessary, in order uniformly to obtain the same results, that the combustion should take place only at the upper part of the piece of charcoal, and by no means extend over the whole of it; otherwise the contact both of the charcoal and of the carbonic acid with the plate of metal destined to receive the electricity will render the experiment irregular. To discover whether the electricity rendered evident in the preceding experiment was to be attributed to chemical action, or to the conversion of the charcoal from the solid to the gaseous state, he examined the flame produced by the combustion of hydrogen. The external part of the flame constantly exhibits positive, and the interior negative electricity, a transfer of electricity taking place between the molecules which are combining, and those which are about to do so. This fact is supported by a great number of experiments on the combustion of phosphorus, sulphur, the metals, alcohol, æther, fat substances, and vegetable matter.

As plants during vegetation exert a chemical action on the atmosphere, sometimes converting its oxygen into carbonic acid, and at others decomposing the carbonic acid already existing in it, the idea suggested itself, that if electricity were developed in these processes of vegetation, their very extensive operation would warrant one in attributing to them a considerable portion of the electricity of the atmosphere.

To investigate this subject, M. Pouillet examined the vegetation of seeds in an insulated situation, having a condenser connected with the soil. Till the germs appeared at the surface, no signs of electricity could be detected, but as vegetation advanced it became very evident. For the success of this experiment it is necessary that the air should be in a state of considerable dryness. When this does not happen to be the case, the apartment must be artificially dried by quicklime, or some other absorbent. It is obvious that the soil could not acquire one electric state without the opposite state in a corresponding degree being communicated to the atmosphere.

If then a languid vegetation on a surface of five or six square feet be capable of producing very decided effects, may we not reasonably conclude, that the influence of the same cause operating over a large portion of the surface of the earth is fully adequate to the production of many of the phænomena which we observe?

A second memoir, by the same author, carries the subject still further, and exhibits other causes, besides the process of vegetation, which contribute to supply the atmosphere with electricity. In the first memoir, he had shown that when two bodies combine, electricity is developed; in the second, he proves that similar phænomena attend the separation of bodies which were previously combined, and he applies this fact to the numerous instances of decomposition which nature is spontaneously producing on the surface of our terraqueous globe.

M. Pouillet, in his experiments connected with this inquiry, employed two processes. The first resembles that adopted by Saussure, in his experiments on evaporation, and consists in connecting one of the disks of the condenser with the heated vessel in which the subject of the experiment is to be placed. By the other process, the heated vessel is dispensed with, and he makes use of one of Fresnel's large lenses to heat the body whilst it rests on a plate of platina. It should be remarked, that when vessels of copper or iron, or of other materials on which the substances under examination can act chemically are employed, the result will be a complication of effect by which the phænomena will sometimes be heightened, and at others neutralized.

The results of his experiments are, first, that by mere evaporation, as before stated, whether it be rapid or slow, no signs of electricity are produced; secondly, that evaporation from an

alkaline solution, however weak, whether it be of soda, potash, barytes, or strontia, leaves the alkali electrified positively; thirdly, that when other solutions, either saline or acid, are employed, evaporation leaves the body which was combined with the water, electrified negatively.

Of the numerous saline solutions which were essayed, that of muriate of soda was naturally the one which excited the greatest interest. It formed no exception to the rule. Hence it can hardly be doubted that evaporation from the surface of the sea forms one of the most important sources of atmospheric electricity. Even lakes and rivers must have their influence, since their waters are never perfectly pure.

Article XIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

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Illustrations of the Anatomy of the Pelvis. By Alexander Monro, MD. FRSE. &c. Part I. Folio. 1*l*. 5*s*.

The Edinburgh Journal of Medical Science. No. I. 6*s*.

Article XIV.

NEW PATENTS.

E. Bowring, Goldsmith-street, London, silk manufacturer, and R. Stamp, Buxted, Sussex, weaver, for improvements in the working, weaving, or preparing silk and other fibrous materials, used in making hats, bonnets, shawls, and other materials.—Nov. 17.

J. Guestier, Fenchurch-buildings, for a mode of making paper from certain substances, which are thereby applicable to that purpose.—Nov. 17.

A. Lamb, Princes'-street, London, and W. Suttill, Old Brompton, flax spinner, for improvements in machinery for preparing, drawing, roving, and spinning flax, hemp, and waste silk.—Nov. 17.

G. Borradaile, Barge-yard, Bucklersbury, merchant and furrier, for an improved method of making or setting up of hats or hat bodies.—Nov. 17.

Count de la Garde, Saint James's-square, Middlesex, for improved machinery for breaking or preparing hemp, flax, and other fibrous materials.—Nov. 24.

J. Eve, Liverpool, engineer, for an improved steam-engine.—Nov. 24.

H. King, Norfolk-street, Commercial-road, Middlesex, master mariner, and W. Kingston, Dock Yard, Portsmouth, master millwright, for improved fids for topmasts, gall and masts, bowsprits, &c.—Nov. 26.

R. J. Tomlinson, Bristol, for frame-work for bedsteads and other purposes.—Nov. 26.

M. Lariviere, Princes'-square, Kennington, mechanist, for certain machinery to be applied to the well-known Stamp's fly presses, or other presses, for the purpose of perforating metal plates, and for the application of such to various useful purposes.—Nov. 28.

W. Pope, Ball-alley, Lombard-street, mathematician, for improvements on wheeled carriages.—Dec. 3.

W. Pope, Ball-alley, mathematician, for improvements in making, mixing, compounding, improving, or altering soap.—Dec. 3.

H. Berry, Abchurch-lane, London, merchant, for an improved method, in different shapes or forms, of securing volatile or other fluids, and concrete or other substances, in various descriptions of bottles and vessels.—Dec. 3.

E. Edmonds, Bradford, clothier, for improvements on machines for scribbling and carding sheep's wool, cotton, or any fibrous articles requiring such process.—Dec. 3.

J. Beever, Manchester, for an improved gun-barrel.—Dec. 3.

E. Luscombe, East Stonehouse, Devon, merchant, for a method of manufacturing or preparing oils extracted from certain vegetable substances, and the application thereof to gas light and other purposes.—Dec. 6.

J. P. Beavan, Clifford-street, Middlesex, for a cement for building and other purposes.—Dec. 7.

Article XV.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
11th Mon.							
Nov. 1	N W	30·10	29·97	56	43	—	—
2	N W	30·10	29·18	55	45	—	08
3	W	29·70	29·10	53	32	—	
4	N W	30·15	29·70	48	28	—	
5	N W	30·15	29·40	54	30	—	02
6	N W	30·45	30·40	58	30	—	—
7	N W	29·58	29·45	54	31	—	—
8	S	29·45	29·25	50	34	—	60
9	W	29·25	29·01	45	32	—	46
10	N E	29 60	29·01	47	35	—	40
11	N W	30·01	29·60	41	32	—	
12	N W	30·15	30·01	38	22	·45	
13	S W	30·15	30·06	38	29	—	
14	N W	30·29	30 06	40	32	—	
15	N	30·38	30·29	43	26	—	
16	S W	30·38	30·30	42	31	—	
17	S	30·30	30·15	48	32	—	15
18	S W	30·15	30·08	58	37	—	08
19	W	30·37	30·08	46	30	—	—
20	S W	30·37	30·02	52	32	—	02
21	S W	30·05	30·02	54	38	—	22
22	N W	30·47	30·05	47	28	—	
23	W	30·46	30·34	48	31	—	—
24	N W	30·41	30·35	53	40	·43	20
25	N W	30·41	30·24	46	32	—	
26	W	30·24	30·12	45	35	—	10
27	W	30·12	29.50	51	39	—	06
28	S W	29·50	29·19	52	40	—	54
29	S W	29·63	29·19	48	33	—	06
30	N W	29·80	29·63	40	22	·32	
		30·47	29·01	58	22	1·20	2·99

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eleventh Month.—1. Fine. 2. Fine: very boisterous all night. 3. A furious wind all day from the westward. 4. Fine. 5. Day fine: night stormy. 6. Cloudy. 7. Fine. 8. Cloudy and fine: rain at night. 9. Fine: rain during the night. 10. Rainy morning: wet day. 11. Cloudy. 12. Fine. 13. White frost. 14. White frost and foggy: fine. 15. Fine. 16. Fine. 17. Drizzly. 18. Drizzly: a slight coloured parhelion about ten, a. m. 19. Fine. 20. Foggy morning: fine day. 21. Rainy. 22. Fine. 23. Cloudy morning: fine afternoon. 24. Rainy. 25. Fine. 26. Rainy. 27. Cloudy. 28. Fine. 29. Rainy. 30. Fine.

RESULTS.

Winds: N, 1; NE, 1; S, 2; SW, 7; W, 6; NW, 13.

Barometer: Mean height

For the month.................................... 29·932 inches.

Thermometer: Mean height

For the month.................................... 40·513°

Evaporation.................................... 1·20 in.

Rain.................................... 2·99

Laboratory, Stratford, Twelfth Month, 24, 1825. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1826.

ARTICLE I.

Extension of a Theorem of Fermat. By Mr. W. G. Horner.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Dec.* 26, 1825.

A PROPERTY of *prime numbers*, which was first stated by Fermat, and which serves so important uses in the analytical division of the circle, and is the basis of the theory of decimal circles, has been demonstrated in two very different ways. The process of Euler is familiar to all, having been repeated by subsequent writers on the theory of numbers. But the manner adopted by Mr. Ivory, in vol. i. of the Math. Repos. possesses important advantages in regard of conciseness, and of the readiness with which the train of reasoning may be extended to *numbers in general*. This circumstance occurred to me some eighteen or twenty months ago, when pursuing a little speculation on decimal circles; the properties of which may be developed to their utmost limit by the aid of this discovery. Grateful to the source of my success, I immediately addressed a copy of my extension of the basis which he had supplied to Mr. Ivory, but am uncertain if it reached its destination. That gentleman can, however, have no objection to its appearance in your respectable work; and being unwilling, not for my own sake only, that my theorem should remain in silence, I shall feel obliged by your early inserting it.

I am, yours very truly,
W. G. HORNER.

Theorem.—If P, p, be prime to each other, and n indicate the number of integers less than p and prime to it, $P^n - 1$ will be divisible by p.

Demonstration.—1. If u is not a multiple of p, neither is u P. For, conceive the three to be resolved into their prime factors; then P, p, have no factors in common; and p contains at least one factor which is not in u, and $\therefore$ not in u P.

2. The remainder of $\frac{u\,\mathrm{P}}{p}$ is commensurate with p, or not, according as u is or is not commensurate with p. For, let the quotient be Q, and the remainder r; then $u\,\mathrm{P} - r = p\,\mathrm{Q}$; and if r and u P be severally divided by any number that measures p, the two remainders will be identical. Let the common remainder be O; then r and u P, and $\therefore$ r and u, since P is prime to p, are each of them commensurate with p. Let the remainder be $>$ O; then each of these is incommensurate with p.

3. If $u - v$ is not a multiple of p, $\frac{u\,\mathrm{P}}{p}$ and $\frac{v\,\mathrm{P}}{p}$ leave different remainders. For if both leave the same remainder r, we shall have $u\,\mathrm{P} - r$, and $v\,\mathrm{P} - r$, both divisible by p; and $\therefore$ their difference $(u - v)\,\mathrm{P}$ will be divisible by p, which is impossible (1).

4. If each term of the series P, 2 P, 3 P, ... $(p - 1)$ P, be divided by p, all the remainders will be distinct (3). And they are $p - 1$ in number. Consequently they comprise every integer $< p$; or, the series of remainders, when progressively arranged, is identical with the natural series of multipliers.

5. Separate this series into two classes, distinguished by having the multipliers prime to p, or commensurate with it. The entire set of remainders in each class, if progressively arranged, will prove identical with the series of multipliers in the same class. For each of these two series is selected from an identical set of numbers (4), and by an identical mode of choice (2).

6. Let the multipliers prime to p be 1, r, r_{1}, r_{11}, &c. and let M p be understood to mean *some multiple of* p. Then each term of the series P, r P, r_1 P, &c. will have one and only one equivalent in a series of equal extent $\mathrm{M}\,p + 1$, $\mathrm{M}\,p + r$, $\mathrm{M}\,p + r_1$, &c. so that the two series are identical. Putting $\mathrm{S} = 1 \times r \times r_1 \times r_{11}$ .... and taking n to indicate the number of terms in each of the two series just described, we shall have for the product of all the terms $\mathrm{P}^n\,\mathrm{S} = \mathrm{M}\,p + \mathrm{S}$. Wherefore $\mathrm{P}^n\,\mathrm{S} - \mathrm{S} = \mathrm{M}\,p$; that is $(\mathrm{P}^n - 1)\,\mathrm{S} = \mathrm{M}\,p$. But S is entirely composed of factors prime to p; and is $\therefore$ prime to p (1). Consequently it must be the other factor $\mathrm{P}^n - 1$ that is divisible by p.—Q. E. D.

Cor.—1. Let m be the number of terms incommensurate with p, in the natural series 1, 2, 3 .... p. Then $(\mathrm{P}^n - 1) \times \mathrm{P}^m = \mathrm{P}^p - \mathrm{P}^m$ being divisible by p, it follows that P^p and P^m, when divided by p, leave the same remainder.

2. If p is a prime number, n is $= p - 1$, and we have $\frac{\mathrm{P}^{p-1} - 1}{p}$ an integer; which is the theorem of Fermat.

3. If p is any number whatever, and its prime factors are a, b, &c.; that is, if $p = a^{\alpha} b^{\beta} \ldots\ldots$ we have $n = (a-1)\, a^{\alpha-1} \times (b-1)\, b^{\beta-1} \times \ldots\ldots$ as is shown by writers on the theory of numbers.

Article II.

Astronomical Observations, 1825. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825.	Stars.	Transits.		
Dec. 15.	—30 Aquarii	21h	54′	07·84″
15.	—2 Aquarii	22	01	29·79
15.	—51 Aquarii	22	15	03·34
15.	—Moon's First or West Limb	22	21	29·54
15.	—166 Aquarii	22	58	45·45
17.	—*b* Piscium	23	11	29·79
17.	—19 Piscium	23	37	31·34
17.	—26 Piscium	23	46	15·28
17.	—ω Piscium	23	50	23·83
17.	—Moon's First or West Limb	23	53	32·96
17.	—36 Piscium	0	07	39·38
17.	—*d* Piscium	0	11	40·34
17.	—45 Piscium	0	16	45·36
17.	—31 Piscium	0	23	26·89
17.	—189 Piscium	0	39	17·26
19.	—75 Piscium	0	57	26·95
19.	—311 Piscium	1	01	00·30
19.	—η Piscium	1	22	13·00
19.	—Moon's First or West Limb	1	26	12·73
19.	—104 Piscium	1	29	58·82
19.	—4 Arietis	1	38	47·38
25.	—μ Gemin.	6	12	28·69
25.	—ν Gemin.	6	18	40·52
25.	—ε Gemin	6	33	16·08
25.	—36 Gemin	6	41	09·45
25.	—Moon's Second or East Limb	6	50	55·25
25.	—δ Gemin	7	09	45·89
25.	—56 Gemin	7	11	42·99
25.	—61 Gemin	7	16	43·07

Dec. 19.	Immersion of Jupiter's first satellite	13h	02′	04″	Mean Time at Bushey.
		13	03	26	Mean Time at Greenwich.
Dec. 31.	Immersion of Jupiter's second satellite	11	24	59	Mean Time at Bushey.
		11	26	20	Mean Time at Greenwich.

Article III.

On the Measurement of Heights by One Barometer, with the requisite Tables. By Mr. J. Nixon.

(*Concluded from* p. 42.)

Use of the Tables.

Table I.—Having given the following observations, to prepare them for the calculation of the altitude.

	Station.	Time.	Pressure.	Mercury.	Air.
(No. 1.)	Gearstones Inn	$9^h\ 15^m$ ..	29·022	.. (46°)	
	Cam Fell.	13 20	28·188	.. (43)	.. 42°
	Gearstones Inn	16 15	29·128	.. (45)	

Temperature of the air at Gearstones at $13^h\ 20^m = 45°$.

Reduction of the Pressures at Gearstones to one Temperature, and Interpolation to $13^h\ 20^m$.

Pressure at........	$16^h\ 15^m$	29·128	(45°)
Add for 1° of difference of the interior therms. = (46°) − (45°)		·003	(*See Table* 1.)
Pressure at........	$16^h\ 15^m$	29·131	(46°)
Ditto.............	9 15	29·022	(46)
Interval	7 0 = $\frac{1}{7}$)	·109	
		·015·6	rise per hour.

Pressure at	9·15	......	29·022
Rise in	4·5	......	·064
Pressure at	13·20	=	29·086 (46°)

The scale being so fixed as to render the corrections for capacity and capillarity superfluous, the observations prepared for calculation will be,

Gearstones Inn.	29·086	(46)	45
Cam Fell	28·188	(43)	42

In the following set of observations, the neutral point of the barometer was 30 inches; the capacity $\frac{1}{45}$; capillarity ·100 inch. Hence the pressures must be diminished by the constant quantity ·522 inch.

Calculation.

Capacity *plus* 1 = $\frac{1}{46}$)	30·000 neutral point.
	·652
Capillarity	− ·100
Constant reduction ..	·552

Observations.

(No. 2.)	Horton Bridge	8^h	40^m	....	29·220 (46)	
	Pen-y-gent. ..	12	10	....	27·640 (35)	33
	Horton Bridge	16	40	....	29·250 (42)	

(No register of the thermometer was kept at Horton Bridge.)

Reduction, &c.

Pressure at Horton Br. at	16^h	40^m	.. 29·250	(42)
Add for 4° of difference of the interior therms. (46) — (42).			·011	(*See Table* 1.)
Pressure at	16^h	40^m	.. 29·261	(46)
Ditto	8	40	.. 29·220	(46)
Interval	8	0	$\frac{1}{8}$) ·041	
			·005·1	rise per hour.

Pressure at	8^h	40^m	......	29·220
Rise in	3	30	......	·018
Pressure at	12	10	=	29·238 (46)

Subtracting from the pressures the constant quantity ·552, we have for calculation,

Horton Bridge (29·238 — ·552) = 28·686 (46°)
Pen-y-gent. .. (27·640 — ·552) = 27·088 (35) 33

Tables II, III, and IV.—The observations being prepared for calculation, enter Table II. with the pressure at the lower station, neglecting the last figure, and take out the corresponding feet, &c. With the same pressure to the nearest inch, find by Table III. the value in feet of the rejected figure (or thousandth part of an inch*). To these two quantities, *add* the feet, &c. answering to the height of the interior thermometer, as given in Table IV.

Find the corresponding quantities for the upper station, and subtract their sum from that obtained for the lower one, and the remainder will be the difference of altitude at 0° F.

Then correct the *sum* of the detached thermometers for the latitude as indicated in Table V, and multiply the corrected sum by the altitude at 0° F. and the decimal fraction ·0012.† The

* It will never cause an error of two feet in the calculation of the altitude to substitute the last figure itself. Gross errors of computation may be detected by estimating ·001 inch of mercury equal to one foot; 1 inch to 1000 feet, &c. See the calculation given, vol. vi. p. 265, where the error exceeds 2000 feet.

† Substituted for $\frac{1}{836}$ = ·0011962. The alteration will never cause a difference of five feet (*additive*) in altitudes of 10000 feet.

difference of level will be equal to the product *added* to the altitude at 0° F.

When the height of the detached thermometer is given only for the upper station, multiply it by 2, and consider the product as the sum of the thermometers. Correct for latitude, and augment the resulting difference of level by the quantity given in Table VI.; or divide the approximate altitude by 500, and add to it the square of the remainder.

Examples.

(No. I.) *Gearstones Inn.* (Lat. 55°) *Cam Fell.*

29·086 (46) 45° 28·188 (43) 42°

7924·5	Table II.	7159·0	42
5·0	III.	7·0	45
116·5	IV.	123·5	—
			87
8046·0		7289.5	−1 Lat.
7289·5			—
			86°

756·5 Difference of altitude at 0° F.
78·0 = 756·5 × 86 × ·0012

834·5 Altitude in feet of Cam Fell* above Gearstones Inn.

(No. 2.) *Horton Bridge.* (Lat. 55°) *Pen-y-gent.*

28·686 (46)	27·088 (35)	33°
		×2
7587·5	6190·0	
5·0	7·0	66
116·5	140·5	−1 Lat.
7709·0	6337·5	65
6337·5		

1371·5 Difference of altitude at 0° F.
107·0 = 1371·5 × 65 × ·0012

1478·5
8·5 See Table II.

1487·0 feet. Altitude of Pen-y-gent above the Ribble at Horton Bridge.

Subjoined are given a few observations by Shuckburgh, Roy, &c. with the heights calculated by the tables, and determined trigonometrically, or by levelling.

* Seven miles from Ingleton on the Hawes road; Cam Fell is nearly four miles ENE of Gearstones; the station on the loftiest point (limestone) is over Cam-houses.

Mount Salêve, base....	28·393	(71½)	73½	2830	..	2832 ft.
——— summit..	25·707	(75½)	64½			
Leith Pier............	29·567	(55¼)	54	801·5	..	803
Arthur's Seat.........	28·704	(51¾)	50½			
Caernarvon Quay.....	29·984	(56½)	55¼	3561	..	3355·5
Snowdon Peak.......	26·271	(42¾)	43			
Keswick Lake (30 ft. ab.)	30·050	(61)	61	2780	..	2778
Summit of Skiddaw....	27·156	(57)	50			

In the tables generally made use of by the author, the calculation is rendered more brief (at some slight expence of accuracy) by the addition in Table II. of a third column, giving the value of $\frac{1}{836}$th part of the preceding one, or the augmentation for 1° of the sum of the detached thermometers. The *difference* of the increment per degree for any two given pressures being multiplied by the sum of the thermometers, we obtain the augmentation in altitude for temperatures above 0°. The quantities in Tables III. and IV. are calculated for the mean temperature of 50° F. To render the method more intelligible, an abstract from the tables is given, together with a type of the calculation.

Table II.

Inches.	Feet.	1°
21·00	0	0 feet
21·01	11·5	0·01
21·02	23·0	0·03
21·03	34·5	0·04
	* * *	
27·08	6190·0	7·40
	* * *	
28·68	7587·5	9·08
	* * *	

Horton Bridge.		*Pen-y-gent.*		
28·686 (46)		27·088 (35)		33°
	1°		1°	×2
7587·5	9·08	6190·0	7·40	
5·5	7·40	8·0		66
130·5		157·5		−1 Lat.
	1·68			
7723·5		6355·5		65
6355·5				×1·68
1368·0				109·0
109·0 = 1·68 × 65				
1477·0				
8·5 = Table VI.				
1485·5 feet. Error − 1·5 feet.				

Table I.

To reduce the Pressures at the Reference Station to one Temperature. Rule; Add the Correction to the colder Bar, or subtract it from the warmer one.

Diff. of temp.	Inches. 30·5	29·5	28·5	27·5	Diff. of temp.	Inches. 30·5	29·5	28·5	27·5
1°	·003	·003	·003	·003	11°	·030	·029	·028	·027
2	·006	·005	·005	·005	12	·033	·032	·031	·030
3	·008	·008	·008	·007	13	·036	·034	·033	·032
4	·011	·011	·010	·010	14	·038	·037	·036	·034
5	·014	·013	·013	·012	15	·041	·040	·038	·037
6	·016	·016	·015	·015	16	·044	·042	·041	·039
7	·019	·019	·018	·017	17	·046	·045	·043	·042
8	·022	·021	·020	·020	18	·049	·048	·046	·044
9	·025	·024	·023	·022	19	·052	·050	·049	·047
10	·027	·026	·026	·025	20	·055	·053	·051	·049

Table II.

Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.
21·00	0	21·37	425	21·74	843	22·11	1254
01	11·5	38	436·5	75	854	12	1265
02	23	39	448	76	865·5	13	1276
03	34·5	21·40	459	77	876·5	14	1287
04	46	41	470·5	78	888	15	1298
05	58	42	482	79	899	16	1309
06	69·5	43	493·5	21·80	910	17	1320
07	81	44	504·5	81	921	18	1331
08	92·5	45	516	82	932·5	19	1342
09	104	46	527·5	83	943·5	22·20	1353
21·10	115·5	47	539	84	955	21	1363·5
11	127	48	550	85	966	22	1374·5
12	138·5	49	561·5	86	977	23	1385·5
13	150	21·50	573	87	988	24	1396·5
14	161·5	51	584	88	999	25	1407·5
15	173	52	595·5	89	1010·5	26	1418·5
16	185	53	606·5	21·90	1021·5	27	1429·5
17	196	54	618	91	1032·5	28	1440
18	208	55	629	92	1044	29	1451
19	219	56	640·5	93	1055	22·30	1462
21·20	231	57	652	94	1066	31	1473
21	242	58	663	95	1077	32	1484
22	253·5	59	674·5	96	1088	33	1495
23	265	21·60	686	97	1099	34	1506
24	276·5	61	697	98	1110	35	1516·5
25	288	62	708	99	1121·5	36	1527·5
26	299·5	63	719·5	22·00	1132·5	37	1538·5
27	311	64	731	01	1143·5	38	1549
28	322·5	65	742	02	1154·5	39	1560
29	334	66	753	03	1165·5	22·40	1571
21·30	345	67	764·5	04	1176·5	41	1582
31	356·5	68	775·5	05	1187·5	42	1593
32	368	69	787	06	1198·5	43	1603·5
33	379·5	21·70	798	07	1210	44	1614·5
34	391	71	809·5	08	1221	45	1625
35	402	72	820·5	09	1232	46	1636
36	413·5	73	832	22·10	1243	47	1647

Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.
22·48	1658	23·08	2299	23·68	2924	24·28	3533
49	1668·5	09	2309·5	69	2934	29	3543
22·50	1679·5	23·10	2320	23·70	2944·5	24·30	3553
51	1690	11	2330·5	71	2955	31	3563
52	1701	12	2341	72	2965	32	3573
53	1712	13	2352	73	2975	33	3583
54	1722·5	14	2362	74	2985·5	34	3593
55	1733·5	15	2373	75	2996	35	3603
56	1744	16	2383	76	3006	36	3613
57	1755	17	2394	77	3016	37	3623
58	1766	18	2404·5	78	3026·5	38	3633
59	1776·5	19	2415	79	3037	39	3643
22·60	1787·5	23·20	2425·5	23·80	3047	24·40	3653
61	1798	21	2436	81	3057	41	3663
62	1809	22	2446	82	3067·5	42	3673
63	1820	23	2457	83	3077·5	43	3683
64	1830·5	24	2467	84	3088	44	3693
65	1841	25	2478	85	3098	45	3703
66	1852	26	2488	86	3108	46	3713
67	1863	27	2498·5	87	3118·5	47	3723
68	1873·5	28	2509	88	3128·5	48	3732·5
69	1884	29	2519·5	89	3139	49	3742·5
22·70	1895	23·30	2530	23·90	3149	24·50	3752·5
71	1905·5	31	2540·5	91	3159	51	3762·5
72	1916·5	32	2551	92	3169	52	3772·5
73	1927	33	2561·5	93	3179·5	53	3782·5
74	1938	34	2572	94	3189·5	54	3792
75	1948·5	35	2582	95	3200	55	3802
76	1959	36	2592·5	96	3210	56	3812
77	1970	37	2603	97	3220	57	3822
78	1980·5	38	2613·5	98	3230	58	3832
79	1991	39	2624	99	3240·5	59	3842
22·80	2002	23·40	2634	24·00	3250·5	24·60	3852
81	2012·5	41	2644·5	01	3260·5	61	3861·5
82	2023	42	2655	02	3271	62	3871·5
83	2034	43	2665·5	03	3281	63	3881·5
84	2044·5	44	2676	04	3291	64	3891
85	2055	45	2686	05	3301	65	3901
86	2066	46	2696·5	06	3311·5	66	3911
87	2076·5	47	2707	07	3321·5	67	3921
88	2087	48	2717·5	08	3331·5	68	3931
89	2098	49	2728	09	3342	69	3940·5
22·90	2108·5	23·50	2738	24·10	3352	24·70	3950·5
91	2119	51	2748·5	11	3362	71	3960
92	2130	52	2759	12	3372	72	3970
93	2140·5	53	2769	13	3382	73	3980
94	2151	54	2779·5	14	3392	74	3990
95	2161·5	55	2790	15	3402	75	3999·5
96	2172	56	2800	16	3412·5	76	4009·5
97	2183	57	2810·5	17	3422·5	77	4019·5
98	2193·5	58	2821	18	3432·5	78	4029
99	2204	59	2831	19	3442·5	79	4039
23·00	2214·5	23·60	2841·5	24·20	3452·5	24·80	4049
01	2225	61	2852	21	3462·5	81	4058·5
02	2235·5	62	2862	22	3473	82	4068·5
03	2246	63	2872·5	23	3483	83	4078
04	2257	64	2882·5	24	3493	84	4088
05	2267·5	65	2893	25	3503	85	4098
06	2278	66	2903	26	3513	86	4107·5
07	2288·5	67	2913·5	27	3523	87	4117·5

Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.
24·88	4127	25·48	4707·5	26·08	5274	26·68	5827·5
89	4137	49	4717	09	5283·5	69	5837
24·90	4147	25·50	4726·5	26·10	5292·5	26·70	5846
91	4156·5	51	4736	11	5302	71	5855
92	4166·5	52	4745·5	12	5311	72	5864
93	4176	53	4755	13	5320·5	73	5873
94	4186	54	4764·5	14	5330	74	5882·5
95	4195·5	55	4774	15	5339	75	5891·5
96	4205·5	56	4783·5	16	5348·5	76	5900·5
97	4215	57	4793	17	5358	77	5909·5
98	4225	58	4802·5	18	5367	78	5919
99	4234.5	59	4812	19	5376·5	79	5928
25·00	4244·5	25·60	4822	26·20	5386	26·80	5937
01	4254	61	4831	21	5395	81	5946
02	4264	62	4841	22	5404	82	5955
03	4273·5	63	4850	23	5413·5	83	5964
04	4283	64	4860	24	5423	84	5973
05	4293	65	4869	25	5432	85	5982
06	4302·5	66	4878·5	26	5441·5	86	5991·5
07	4312·5	67	4888	27	5450·5	87	6000·5
08	4322	68	4897·5	28	5460	88	6009·5
09	4332	69	4907	29	5469	89	6018·5
25·10	4341·5	25·70	4916·5	26·30	5478·5	26·90	6027·5
11	4351	71	4926	31	5487·5	91	6036·5
12	4361	72	4935·5	32	5497	92	6045·5
13	4370·5	73	4945	33	5506	93	6055
14	4380	74	4954·5	34	5515·5	94	6064
15	4390	75	4964	35	5524·5	95	6073
16	4399·5	76	4973·5	36	5534	96	6082
17	4409·5	77	4983	37	5543	97	6091
18	4419	78	4992	38	5552·5	98	6100
19	4428·5	79	5002	39	5561·5	99	6109
25·20	4438·5	25·80	5011	26·40	5571	27·00	6118
21	4448	81	5020·5	41	5580	01	6127
22	4457·5	82	5030	42	5589	02	6136
23	4467	83	5039·5	43	5598·5	03	6145
24	4477	84	5049	44	5607·5	04	6154
25	4486·5	85	5058	45	5617	05	6163
26	4496	86	5067·5	46	5626	06	6172
27	4506	87	5077	47	5635	07	6181
28	4515·5	88	5086·5	48	5644·5	08	6190
29	4525	89	5096	49	5653·5	09	6199
25·30	4535	25·90	5105·5	26·50	5663	27·10	6208
31	4544·5	91	5115	51	5672	11	6217
32	4554	92	5124	52	5681	12	6226
33	4563·5	93	5133·5	53	5690·5	13	6235
34	4573	94	5143	54	5699·5	14	6244
35	4583	95	5152	55	5709	15	6253
36	4592·5	96	5161·5	56	5718	16	6262
37	4602	97	5171	57	5727	17	6270·5
38	4611·5	98	5180·5	58	5736	18	6279·5
39	4621	99	5190	59	5745·5	19	6288·5
25·40	4631	26·00	5199	26·60	5754·5	27·20	6297·5
41	4640·5	01	5208·5	61	5763·5	21	6306·5
42	4650	02	5218	62	5773	22	6315·5
43	4659·5	03	5227	63	5782	23	6324·5
44	4669	04	5236·5	64	5791	24	6333·5
45	4678·5	05	5246	65	5800	25	6342
46	4688	06	5255	66	5809·5	26	6351
47	4698	07	5264·5	67	5818·5	27	6360

Inches.	Feet.	Inches	Feet.	Inches.	Feet.	Inches.	Feet.
27·28	6369	27·88	6898·5	28·48	7417	29·08	7924·5
29	6378	89	6907·5	49	7425·5	09	7933
27·30	6387	27·90	6916	28·50	7434	29·10	7941·5
31	6396	91	6925	51	7442·5	11	7950
32	6405	92	6933·5	52	7451	12	7958
33	6413·5	93	6942	53	7460	13	7966·5
34	6422·5	94	6951	54	7468	14	7975
35	6431·5	95	6960	55	7477	15	7983
36	6440·5	96	6968·5	56	7485	16	7991·5
37	6449	97	6977	57	7494	17	8000
38	6458	98	6986	58	7502·5	18	8008
39	6467	99	6994·5	59	7511	19	8016·5
27·40	6476	28·00	7003	28·60	7519·5	29·20	8025
41	6485	01	7012	61	7528	21	8033
42	6493·5	02	7020·5	62	7536·5	22	8041·5
43	6502·5	03	7029	63	7545	23	8050
44	6511·5	04	7038	64	7553·5	24	8058
45	6520	05	7046·5	65	7562	25	8066·5
46	6529	06	7055·5	66	7570·5	26	8075
47	6538	07	7064	67	7579	27	8083
48	6547	08	7072·5	68	7587·5	28	8091·5
49	6555.5	09	7081·5	69	7596	29	8100
27·50	6564·5	28·10	7090	28·70	7604·5	29·30	8108
51	6573·5	11	7098·5	71	7613	31	8116·5
52	6582	12	7107·5	72	7621	32	8124·5
53	6591	13	7116	73	7630	33	8133
54	6600	14	7124·5	74	7638	34	8141
55	6609	15	7133	75	7646·5	35	8149·5
56	6617·5	16	7142	76	7655	36	8158
57	6626·5	17	7150·5	77	7663·5	37	8166
58	6635	18	7159	78	7672	38	8174·5
59	6644	19	7168	79	7680·5	39	8182·5
27·60	6653	28·20	7176·5	28·80	7689	29·40	8191
61	6662	21	7185	81	7697·5	41	8199
62	6670·5	22	7194	82	7706	42	8207·5
63	6679·5	23	7202·5	83	7714·5	43	8216
64	6688	24	9211	84	7723	44	8224
65	6697	25	7219·5	85	7731	45	8232·5
66	6706	26	7228	86	7739·5	46	8240·5
67	6714·5	27	7237	87	7748	47	8249
68	6723·5	28	7245·5	88	7756·5	48	8257
69	6732	29	7254	89	7765	49	8265·5
27·70	6741	28·30	7262·5	28·90	7773·5	29·50	8273·5
71	6750	31	7271	91	7782	51	8282
72	6758·5	32	7280	92	7790	52	8290
73	6767·5	33	7288·5	93	7798·5	53	8298·5
74	6776	34	7297	94	7807	54	8306·5
75	6785	35	7305·5	95	7815·5	55	8315
76	6793·5	36	7314	96	7824	56	8323
77	6802·5	37	7322·5	97	7832·5	57	8331·5
78	6811	38	7331·5	98	7841	58	8339·5
79	6820	39	7340	99	7849	59	8348
27·80	6829	28·40	7348·5	29·00	7857·5	29·60	8356
81	6837·5	41	7357	01	7866	61	8364
82	6846	42	7365·5	02	7874·5	62	8372·5
83	6855	43	7374	03	7882·5	63	8380·5
84	6864	44	7383	04	7891	64	8389
85	6872·5	45	7391·5	05	7899·5	65	8397
86	6881	46	7400	06	7908	66	8405
87	6890	47	7408·5	07	7916	67	8413·5

Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.
29·68	8421·5	30·01	8691	30·34	8957	30·67	9220·5
69	8430	02	8699	35	8965	68	9228·5
29·70	8438	03	8707	36	8973	69	9236·5
71	8446	04	8715	37	8981	30·70	9244·5
72	8454·5	05	8723·5	38	8989	71	9252
73	8462·5	06	8731·5	39	8997	72	9260
74	8471	07	8739·5	30·40	9005	73	9268
75	8479	08	8748	41	9013	74	9276
76	8487	09	8756	42	9021	75	9284
77	8495·5	30·10	8764	43	9029	76	9292
78	8503·5	11	8772	44	9037	77	9300
79	8512	12	8780	45	9045	78	9307·5
29·80	8520	13	8788	46	9053	79	9315·5
81	8528	14	8796	47	9061	30·80	9323·5
82	8536·5	15	8804	48	9069	81	9331·5
83	8544·5	16	8812·5	49	9077	82	9339
84	8552·5	17	8820·5	30·50	9085	83	9347
85	8561	18	8828·5	51	9093	84	9355
86	8569	19	8836·5	52	9101	85	9363
87	8577	30·20	8844·5	53	9109	86	9371
88	8585	21	8852·5	54	9117	87	9378·5
89	8593·5	22	8860·5	55	9125	88	9386·5
29·90	8601·5	23	8869	56	9133	89	9394·5
91	8609·5	24	8877	57	9141	30·90	9402·5
92	8618	25	8885	58	9149	91	9410
93	8626	26	8893	59	9157	92	9418
94	8634	27	8901	30·60	9165	93	9426
95	8642	28	8909	61	9173	94	9434
96	8650	29	8917	62	9181	95	9442
97	8658·5	30·30	8925	63	9189	96	9449·5
98	8666·5	31	8933	64	9196·5	97	9457·5
99	8674·5	32	8941	65	9204·5	98	9465·5
30·00	8683	33	8949	66	9212·5	30·99	9473

Table III.

Pressure.	Inches. ·001	·002	·003	·004	·005	·006	·007	·008	·009
	Feet.								
21 in.	1·0	2·5	3·5	4·5	6·0	7·0	8·0	9·5	10·5
22	1·0	2·0	3·5	4·5	5·5	6·5	8·0	9·0	10·0
23	1·0	2·0	3·0	4·0	5·5	6·5	7·5	8·5	9·5
24	1·0	2·0	3·0	4·0	5·0	6·0	7·0	8·0	9·0
25	1·0	2·0	3·0	4·0	5·0	6·0	7·0	8·0	8·5
26	1·0	2·0	3·0	3·5	4·5	5·5	6·5	7·5	8·5
27	1·0	2·0	2·5	3·5	4·5	5·5	6·5	7·0	8·0
28	1·0	1·5	2·5	3·5	4·5	5·0	6·0	7·0	8·0
29	1·0	1·5	2·5	3·5	4·0	5·0	6·0	6·5	7·5
30	1·0	1·5	2·5	3·0	4·0	5·0	5·5	6·5	7·5
31	1·0	1·5	2·5	3·0	4·0	4·5	5·5	6·5	7·0

Table IV.

For the interior Thermometer.

	Feet.		Feet.		Feet.		Feet.		Feet.		Feet.
10°	195·0	25°	162·5	40°	130·0	55°	97·0	70°	65·0	85°	32·5
10½	194·0	25½	161·5	40½	128·5	55½	96·0	70½	63·5	85½	31·5
11	193·0	26	160·0	41	127·5	56	95·0	71	62·5	86	30·0
11½	192·0	26½	159·0	41½	126·5	56½	94·0	71½	61·5	86½	29·0
12	190·5	27	158·0	42	125·5	57	93·0	72	60·5	87	28·0
12½	189·5	27½	157·0	42½	124·5	57½	92·0	72½	59·5	87½	27·0
13	188·5	28	156·0	43	123·5	58	90·5	73	58·5	88	26·0
13½	187·5	28½	155·0	43½	122·0	58½	89·5	73½	57·0	88½	25·0
14	186·5	29	153·5	44	121·0	59	88·5	74	56·0	89	24·0
14½	185·5	29½	152·5	44½	120·0	59½	87·5	74½	55·0	89½	22·5
15	184·0	30	151·5	45	119·0	60	86·5	75	54·0	90	21·5
15½	183·0	30½	150·5	45½	118·0	60½	85·5	75½	53·0	90½	20·5
16	182·0	31	149·5	46	116·5	61	84·0	76	52·0	91	19·5
16½	181·0	31½	148·5	46½	115·5	61½	83·0	76½	50·5	91½	18·5
17	180·0	32	147·0	47	114·5	62	82·0	77	49·5	92	17·5
17½	179·0	32½	146·0	47½	113·5	62½	81·0	77½	48·5	92½	16·0
18	177·5	33	145·0	48	112·5	63	80·0	78	47·5	93	15·0
18½	176·5	33½	144·0	48½	111·5	63½	79.0	78½	46·5	93½	14·0
19	175·5	34	143·0	49	110·0	64	78 0	79	45·5	94	13·0
19½	174·5	34½	141·5	49½	109·0	64½	76·5	79½	44·5	94½	12·0
20	173·5	35	140·5	50	108·0	65	75·5	80	43·0	95	11·0
20½	172·0	35½	139·5	50½	107·0	65½	74·5	80½	42·0	95½	9·5
21	171·0	36	138·5	51	106·0	66	73·5	81	41·0	96	8·5
21½	170·0	36½	137·5	51½	105·0	66½	72·5	81½	40·0	96½	7·5
22	169·0	37	136·5	52	103·5	67	71·5	82	39·0	97	6·5
22½	168·0	37½	135·0	52½	102·5	67½	70·0	82½	38·0	97½	5·5
23	167·0	38	134·0	53	101·5	68	69·0	83	36·5	98	4·5
23½	165·5	38½	133·0	53½	100·5	68½	68·0	83½	35·5	98½	3·0
24	164·5	39	132·0	54	99·5	69	67·0	84	34·5	99	2·0
24½	163·5	39½	131·0	54½	98·5	69½	66·0	84½	33·5	99½	1·0

Table V.

For the Latitude.

Equation of the Sum of the detached Thermometers.

In lat.	to	Add	In lat.	to	Subtract
0°	5°	2¾° F.	45°	46°	0° F.
6	13	2½	47	48	0¼
14	18	2¼	49	51	0½
19	22	2	52	54	0¾
23	26	1¾	55	57	1
27	29	1½	58	60	1¼
30	32	1¼	61	63	1½
33	35	1	64	67	1¾
36	38	0¾	68	71	2
39	41	0½	72	76	2¼
42	43	0¼	77	84	2½
44	45	0	85	90	2¼

TABLE VI.

Augmentation of the Altitude calculated with double the detached Thermometer at the upper Station.

Alt.	Feet.	Alt.	Feet.	Alt.	Feet.	Alt.	Feet.
500	+ 1	1871	+ 14	2598	+ 27	3162	+ 40
707	2	1937	15	2646	28	3202	41
866	3	2000	16	2693	29	3240	42
1000	4	2062	17	2739	30	3279	43
1118	5	2121	18	2784	31	3317	44
1225	6	2180	19	2828	32	3354	45
1323	7	2238	20	2872	33	3391	46
1414	8	2291	21	2916	34	3428	47
1500	9	2345	22	2958	35	3464	48
1581	10	2398	23	3000	36	3500	49
1658	11	2450	24	3041	37	3536	50
1732	12	2500	25	3082	38	3571	51
1803	13	2550	26	3123	39	3606	52

ARTICLE IV.

Meteorological Table kept at Bushey Heath in 1825.
By Col. Beaufoy, FRS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

THE barometer, thermometer, and winds, were observed at nine o'clock in the morning, at which hour the temperature of the external air is nearly the same as the mean temperature: see Columns 3 and 8.

The coldest day was March 17, thermometer 24·3°; and the hottest, July 19, thermometer 92·5°.

					Six's Therm.			Winds.							
Months.	Barom.	Ther.	Rain.	Evap.	Min.	Max.	Mean.	N.	NE.	E.	SE.	S.	SW.	W.	NW.
	Inches.		Inches.	Inches.											
Jan....	29·737	37·4	0·778	1·20	34·3°	42·1°	38·2°	2	4	1	1	0	11	3	9
Feb. ...	29·658	36·1	0·810	1·28	34·8	42·3	38·1	1	2	2	6	1	7	4	4
March..	29·289	38·6	1·454	2·07	34·2	45·7	40·0	1	13	2	3	0	7	1	4
April...	29·870	49·3	2·333	4·21	42·1	58·5	50·3	0	8	2	4	2	8	2	4
May ...	29·496	53·6	3·585	3·79	47·9	62·6	55·3	1	13	2	3	0	9	1	2
June. ..	29·531	59·1	1·467	3·07	57·1	66·3	61·7	1	7	0	1	0	15	1	4
July. ..	29·671	64·8	0·062	6·43	56·7	74·8	65·8	1	16	2	1	0	4	3	4
August.	29·475	61·0	2·725	4·00	56·2	69·1	62·6	1	6	3	1	0	8	7	5
Sept. ..	29·429	59·7	3·608	3·00	55·2	65·9	60·5	0	5	2	3	1	10	2	7
Oct	29·500	50·0	2·404	2·04	46·4	56·0	51·2	1	2	0	4	1	14	1	8
Nov....	29·238	40·4	2·884	1·45	36·7	46·9	41·8	1	3	0	1	0	10	5	10
Dec	29·074	37·4	3·200	1·20	37·4	44·0	37·4	0	5	2	4	0	9	3	8
Year.	29·497	48·9	25·31	33·74	44·91	51·19	50·05	10	84	18	32	5	112	33	69

Article V.

On new Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat. By M. Faraday, F.R.S. Cor. Mem. Royal Academy of Sciences of Paris, &c.

(*Concluded from* p. 50.)

Among the liquid products obtained from the original fluid was one which, procured as before mentioned, by submitting to 0° the portion distilling over at 180° or 190°, corresponded with the substance already described, as to boiling points, but differed from it in remaining fluid at low temperatures; and I was desirous of comparing the two together. I had no means of separating this body from the bi-carburet of hydrogen, of which it would of course be a saturated solution at 0°. Its boiling point was very constantly 186°. In its general characters of solubility, combustibility, action of potassium, &c. it agreed with the substance already described. Its specific gravity was 0·86 at 60°. When raised in vapour 1·11 grain of it gave 1·573 cubic inches of vapour at 212°, equal to 1·212 cubic inch at 60°. Hence 100 cubic inches would weigh about 91·6 grains, and its specific gravity would be 43·25 nearly. In another experiment, 1·72 grain gave 2·4 cubic inches at 212°, equal to 1·849 cubic inch at 60°; from which the weight of 100 cubic inches would be deduced as 93 grains; and its specific gravity to hydrogen as 44 to 1. Hence probably the reason why, experimentally, the specific gravity of bi-carburet of hydrogen in vapour was found higher, than by theory it would appear to be when pure.

Sulphuric acid acted much more powerfully upon this substance than upon the bi-carburet: great heat was evolved, much discolouration occasioned, and a separation took place into a thick black acid, and a yellow lighter liquid, resisting any further action at common temperatures.

0·64 grain of this substance was passed over heated oxide of copper; 4·51 cubic inches of carbonic acid gas were obtained, and 0·6 grain of water. The carbonic acid and water are equivalent to

Carbon	0·573176 or	8·764
Hydrogen.	0 066666	1·

but as the substance must have contained much bi-carburet of hydrogen, it is evident that, if in a pure state, the carbon would fall far short of the above quantity, and the compound would approximate of course to a simple carburet of hydrogen containing single proportionals.

New Carburet of Hydrogen.

Of the various other products from the condensed liquor, the next most definite to the bi-carburet of hydrogen appears to be that which is most volatile. If a portion of the original liquid be warmed by the hand, or otherwise, and the vapour which passes off be passed through a tube at 0°, very little uncondensed vapour will go on to the mercurial trough; but there will be found after a time a portion of fluid in the tube, distinguished by the following properties. Though a liquid at 0°, it upon slight elevation of temperature begins to boil, and before it has attained 32°, is all resolved into vapour or gas, which may be received and preserved over mercury.

This gas is very combustible, and burns with a brilliant flame. The specific gravity of the portion I obtained was between 27 and 28, hydrogen being 1: for 39 cubic inches introduced into an exhausted glass globe were found to increase its weight 22·4 grains at 60° F. bar. 29·94. Hence 100 cubic inches weigh nearly 57·44 grains.

When cooled to 0° it condensed again, and inclosed in this state in a tube of known capacity, and hermetically sealed up, the bulk of a given weight of the substance at common temperatures was ascertained. This compared with water gave the specific gravity of the liquid as 0·627 at 54°. It is therefore among solids or liquids the lightest body known.

This gas or vapour when agitated with water is absorbed in small quantities. Alcohol dissolves it in large quantity; and a solution is obtained, which, upon the addition of water, effervesces, and a considerable quantity of the gas is liberated. The alcoholic solution has a peculiar taste, and is neutral to test papers.

Olive oil dissolves about six volumes of the gas.

Solution of alkali does not affect it; nor does muriatic acid.

Sulphuric acid condenses the gas in very large quantity; one volume of the acid condensing above 100 volumes of the vapour. Sometimes the condensation is perfect, at other times a small quantity of residual gas is left, which burns with a pale blue flame, and seems to be a product of too rapid action. Great heat is produced during the action; no sulphurous acid is formed; the acid is much blackened, has a peculiar odour, and upon dilution generally becomes turbid, but no gas is evolved. A permanent compound of the acid with carbon and hydrogen is produced, and enters as before mentioned into combination with bases.

A mixture of two volumes of this vapour with 14 volumes of pure oxygen was made, and a portion detonated in an eudiometer tube. 8·8 volumes of the mixture diminished by the spark to 5·7 volumes, and these by solution of potash to 1·4

volume, which was oxygen. Hence 7·4 volumes had been consumed, consisting of

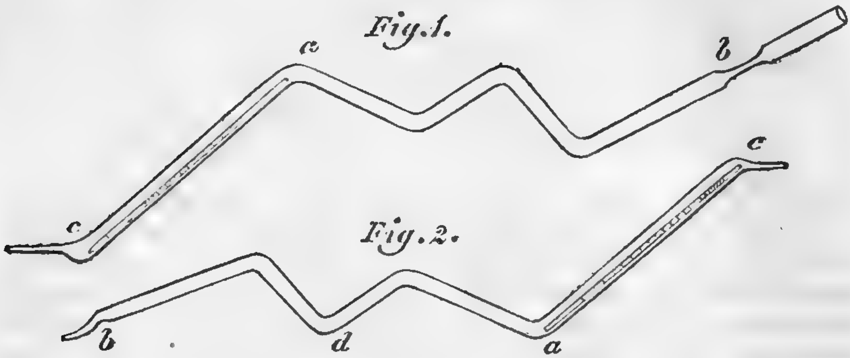
Vapour of substance	1·1
Oxygen	6·3
Carbonic acid formed	4·3
Oxygen in carbonic acid	4·3
Oxygen combining with hydrogen	2·0
Diminution by spark	3·1

This is nearly as if 1 volume of the vapour or gas had required 6 volumes of oxygen, had consumed 4 of them in producing 4 of carbonic acid gas, and had occupied the other 2 by 4 of hydrogen to form water. Upon which view, 4 volumes or proportionals of hydrogen = 4, are combined with 4 proportionals of carbon = 24, to form one volume of the vapour, the specific gravity of which would therefore be 28. Now this is but little removed from the actual specific gravity obtained by the preceding experiments; and knowing that this vapour must contain small portions of other substances in solution, there appears no reason to doubt that, if obtained pure, it would be found thus constituted.

As the proportions of the elements in this vapour appear to be the same as in olefiant gas, it became interesting to ascertain whether chlorine had the same action upon it as on the latter body. Chlorine and the vapour were therefore mixed in an exhausted retort: rapid combination took place, much heat was evolved, and a liquor produced resembling hydro-chloride of carbon, or the substance obtained by the same process from olefiant gas. It was transparent, colourless, and heavier than water. It had the same sweet taste, but accompanied by an after aromatic bitterness, very persistent. Further, it was composed of nearly equal volumes of the vapour and chlorine: it could not therefore be the same as the hydro-chloride of carbon from olefiant gas, since it contained twice as much carbon and hydrogen. It was therefore treated with excess of chlorine in sun-light: action slowly took place, more chlorine combined with the substance, muriatic acid was formed, and ultimately a fluid tenacious triple compound of chlorine, carbon, and hydrogen was obtained, but no chloride of carbon. This is a remarkable circumstance, and assists in showing, that though the elements are the same, and in the same proportions as in olefiant gas, they are in a very different state of combination.

The tension of the most volatile part of the condensed oil gas liquid, and indeed of the substance next beneath olefiant gas in elasticity existing in the mixture constituting oil gas, appears to be equal to about four atmospheres at the temperature of 60°.

To ascertain this a tube was prepared, like the one delineated in the sketch, Fig. 1, containing a mercurial guage at *a*, *c*, and the extremities being open. It was then cooled to 0° from *a* to *b*,



and in that state made the receiver into which the first product from a portion of the original fluid was distilled. The part at *b* was then closed by a spirit lamp; and having raised enough vapour to make it issue at *c*, that was also closed. The instrument now placed as at Fig. 2, had *a* and *d* cooled to 0°, whilst the fluid collected in *b* was warmed by the hand or the air; and when a portion had collected in *d* sufficient for the purpose, the whole instrument was immersed in water at 60°; and before the vapour had returned and been all dissolved by the liquid at *b*, the pressure upon the guage within was noted. Sometimes the fluid at *d* was rectified by warming that part of the tube, and cooling *a* only, the reabsorption at *b* being prevented or rather retarded, in consequence of the superior levity of the fluid at *d*, so that the first portions which returned to *b* lay upon it in a stratum, and prevented sudden solution in the mass below. This difference in specific gravity was easily seen upon agitation, in consequence of the striæ produced during the mixture.

Proceeding in this way it was found, as before stated, that the highest elastic power that could be obtained from the substances in the tube, was about four atmospheres at 60°; and as there seems no reason to doubt, but that portions of the most volatile substances in oil gas beneath olefiant gas were contained in the fluid, inasmuch as even olefiant gas itself is dissolved by it in small proportions, it may be presumed that there is no substance in oil gas much more volatile than the one requiring a pressure of four atmospheres at 60°, except the well-known compounds; or, in other words, that there is not a series of substances passing upwards from this body to olefiant gas, and possessing every intermediate degree of elasticity, as there

Note.—The particular inclination of the parts of the tube one to another was given, that the fluid when required might be returned from *a* to *d* without passing on to *b*.

seems to be from this body downwards, to compounds requiring 250° or 300° for their ebullition.

In reference to these more volatile products, I may state that I have frequently observed a substance come over in small quantity, rising with the vapour which boils off at 50° or 60°, and crystallizing in spiculæ in the receiver at 0°. A temperature of 8° or 10° causes its fusion and disappearance. It is doubtless a peculiar and definite body, but the quantity is extremely small, or else it is very soluble in the accompanying fluids. I have not yet been able to separate it, or examine it minutely.

I ventured some time since upon the condensation of various gases,* to suggest the possibility of forming a vapour lamp, which containing a brilliantly combustible substance, liquid at a pressure of two, three, or four atmospheres at common temperatures, but a vapour at less pressure, should furnish a constant light for a length of time, without requiring high, or involving inconstant pressure. Such a lamp I have now formed, feeding it with the substance just described; and though at present it is only a matter of curiosity, and perhaps may continue so, yet there is a possibility that processes may be devised, by which the substance may be formed in larger quantity, and render an application of this kind practically useful.

On the remaining Portions of the condensed Oil Gas Liquor.

It has been before mentioned, that by repeated distillations various products were obtained, boiling within limits of temperature which did not vary much; and which when distilled were not resolved into other portions, differing far from each other in volatility, as always happened in the earlier distillations. Though conscious that these were mixtures, perhaps of unknown bodies, and certainly in unknown proportions; yet experiments were made on their composition by passing them over oxide of copper, in hopes of results which might assist in suggesting correct views of their nature. They all appeared to be binary compounds of carbon and hydrogen, and the following table exhibits the proportions obtained: the first column expressing the boiling temperature at which the products were distilled, as before mentioned; the second the hydrogen, made a constant quantity; and the third the carbon.

140°	1	7·58
150	1	8·38
160	1	7·90
176	1	8·25
190	1	8·76
200	1	9·17
210	1	8·91
220	1	8·46

* Quarterly Journal of Science, xvi. 240.

These substances generally possess the properties before described, as belonging to the bi-carburet of hydrogen. They all resist the action of alkali, even that which requires a temperature above 250° for its ebullition; and in that point are strongly distinguished from the oils from which they are produced. Sulphuric acid acts upon them instantly with phenomena already briefly referred to.

Dr. Henry, whilst detailing the results of his numerous and exact experiments in papers laid before the Royal Society, mentions in that read Feb. 22, 1821,* the discovery made by Mr. Dalton, of a vapour in oil gas of greater specific gravity than olefiant gas, requiring much more oxygen for its combustion, but yet condensible by chlorine. Mr. Dalton appears to consider all that was condensible by chlorine as a new and constant compound of carbon and hydrogen; but Dr. Henry, who had observed that the proportion of oxygen required for its combustion varied from 4·5 to 5 volumes, and the quantity of carbonic acid produced, from 2·5 to 3 volumes, was inclined to consider it as a mixture of the vapour of a highly volatile oil with the olefiant and other combustible gases; and he further mentions, that naphtha in contact with hydrogen gas will send up such a vapour; and that he has been informed, that when oil gas was condensed in Gordon's lamp, it deposited a portion of highly volatile oil.

A writer in the *Annals of Philosophy*, N. S. iii. 37, has deduced from Dr. Henry's experiments, that the substance, the existence of which was pointed out by Mr. Dalton, was not a new gas sui generis, " but a modification of olefiant gas, constituted of the same elements as that fluid, and in the same proportions, with this only difference, that the compound atoms are triple instead of double :" and Dr. Thomson has adopted this opinion in his Principles of Chemistry. This, I believe, is the first time that two gaseous compounds have been supposed to exist, differing from each other in nothing but density; and though the proportion of 3 to 2 is not confirmed, yet the more important part of the statement is, by the existence of the compound, described at page 96, which though composed of carbon and hydrogen in the same proportion as in olefiant gas, is of double the density.†

* Philosophical Transactions.

† In reference to the existence of bodies composed of the same elements and in the same proportions, but differing in their qualities, it may be observed, that now we are taught to look for them, they will probably multiply upon us. I had occasion formerly to describe a compound of olefiant gas and iodine (Phil. Trans. cxi. 72), which upon analysis yielded one proportional of iodine, two proportionals of carbon, and two of hydrogen (Quarterly Journal, xiii. 429). M. Serrulas, by the action of potassium upon an alcoholic solution of iodine, obtained a compound decidedly different from the preced-

It is evident that the vapour observed by Mr. Dalton and Dr. Henry must have contained not only this compound, and a portion of the bi-carburet of hydrogen, but also portions of the other, as yet apparently indefinite substances; and there can be no doubt that the quantity of these vapours will vary from the point of full saturation of the gas, when standing over water and oil, to unknown, but much smaller proportions. It is therefore an object in the analysis of oil and coal gas, to possess means by which their presence and quantity may be ascertained; and this I find may be done with considerable exactness by the use of sulphuric acid, oil, &c. in consequence of their solvent power over them.

Sulphuric acid is in this respect a very excellent agent. It acts upon all these substances instantly, evolving no sulphurous acid; and though, when the quantity of substance is considerable as compared with the acid, a body is left undecomposed by, or uncombined with the acid, and volatile, so as constantly to afford a certain portion of vapour; yet when the original substance is in small quantity, as where it exists in vapour in a given volume of gas, this does not interfere, in consequence of the solubility of the vapour of the new compound produced by the action of the acid in the acid itself in small quantities; and I found that when one volume of the vapour of any of the products of the oil gas liquor was acted upon, either alone, or mixed with 1, 2, 3, 4, up to 12 volumes of air, oxygen or hydrogen, by from half a volume to a volume of sulphuric acid, it was entirely absorbed and removed.

When olefiant gas is present, additional care is required in analytical experiments, in consequence of the gradual combination of the olefiant gas with the sulphuric acid. I found that one volume of sulphuric acid in abundance of olefiant gas, absorbed about seven volumes in 24 hours in the dull light of a room; sunshine seemed to increase the action a little. When the olefiant gas was diluted with air or hydrogen, the quantity

ing in its properties; yet when analysed, it yielded the same elements in the same proportions (Ann. de Chimie, xx. 245, xxii. 172).

Again. MM. Liebig and Gay Lussac, after an elaborate and beautiful investigation of the nature of fulminating compounds of silver, mercury, &c. were led to the conclusion that they were salts, containing a new acid, and owed their explosive powers to the facility with which the elements of this acid separated from each other (Annales de Chimie, xxiv. 294, xxv. 285). The acid itself being composed of one proportional of oxygen, one of nitrogen, and two of carbon, is equivalent to a proportional of oxygen + a proportional of cyanogen, and is therefore considered as a true cyanic acid. But M. Wohler, by deflagrating together a mixture of ferro-prussiate of potash and nitre, has formed a salt, which, according to his analysis, is a true cyanate of potash. The acid consists of one proportion of oxygen, one of nitrogen, and two of carbon. It may be transferred to various other bases, as the earths, the oxides of lead, silver, &c.; but the salts formed have nothing in common with the similar salts of MM. Liebig and Gay Lussac, except their composition (Gilbert's Annalen, lxxiii. 157; Ann. de Chimie, xxvii. 190). M. Gay Lussac observes, that if the analysis be correct, the difference can only be accounted for by admitting a different mode of combination.

absorbed in a given time was much diminished; and in those cases it was hardly appreciable in two hours; a length of time which appears to be quite sufficient for the removal of any of the peculiar vapours from oil or coal gas.

My mode of operating was generally in glass tubes over clean mercury,* introducing the gas, vapour or mixture, and then throwing up the sulphuric acid by means of a bent tube with a bulb blown in it, passing the acid through the mercury by the force of the mouth. The following results are given as illustrations of the process:—

Oil gas from a gasometer.

		in 8′	in 1 hour.	2 hours.	diminution.
188 vol.	+ 9·5 vol. sulphuric acid diminished to	155·	148·5	146·4	22·12 per cent.
107	+ 13·	88·5	84·5	82·0	23·33
138	+ 5·2	113·7	108·0	106·5	22·82

Oil gas from Gordon's lamp.

		15′	30′	3 hours.	
214	+ 6·8	183·3	180·8	176·	17·75
159	+ 5·9	137·5	136·0	130·4	17·98
113	+ 12·2	98·0	96·0	92·0	18·58

Coal gas of poor quality.

548·6	+ 27·6	533·3	529·2	529·	3·57
273·6	+ 27·8	267·9	266·	266·	2·78
190·6	+ 13·1	186·	184·2	184·1	3·41

Oil may also be used in a similar manner for the separation of these vapours. It condenses about six volumes of the most elastic vapour at common temperatures, and it dissolves with greater facility the vapour of those liquids requiring higher temperatures for their ebullition. I found that in mixtures made with air or oxygen for detonation, I could readily separate the vapour by means of olive oil; and when olefiant and other gases were present, its solvent power over them was prevented, by first agitating the oil with olefiant gas or with a portion of the gas to saturate it, and then using it for the removal of the vapours.

In the same way some of the more fixed essential oils may be used, as *dry* oil of turpentine; and even a portion of the condensed liquor itself, as that part which requires a temperature of 220° or 230° for its ebullition: care being taken to estimate the expansion of the gas by the vapour of the liquid, which may readily be done by a known portion of common air preserved over the liquid as a standard.

With reference to the proportions of the different substances in the liquid as obtained by condensation of oil gas, it is extremely difficult to obtain any thing like precise results, in

* If the mercury contain oxidizable metals, the sulphuric acid acts upon it, and evolves sulphurous acid gas. It may be cleaned sufficiently by being left in contact with sulphuric acid for 24 hours, agitating it frequently at intervals.

consequence of the immense number of rectifications required to separate the more volatile from the less volatile portions; but the following table will furnish an approximation. It contains the loss of 100 parts by weight of the original fluid by evaporation in a flask for every 10° in elevation of temperature, the substance being retained in a state of ebullition.

100 parts at	58°	parts.	differ.
had lost at	70	1·1	
			1·9
	80	3·0	
			2·2
	90	5·2	
			2·5
	100	7·7	
			2·4
	110	10·1	
			3·1
	120	13·2	
			2·9
	130	16·1	
			3·2
	140	19·3	
			3·1
	150	22·4	
			3·2
	160	25·6	
			3·4
	170	29·0	
			15·7
	180	44·7	
			23·4
	190	68·1	
			16·1
	200	84·2	
			7·4
	210	91·6	
			3·7
	220	95·3	
			1·3
	230	96·6	

The residue 3·4 parts was dissipated before 250° with slight decomposition. The third column expresses the quantity volatilized between each 10°, and indicates the existence of what has been described as bi-carburet of hydrogen in considerable quantity.

The importance of these vapours in oil gas, as contributing to its very high illuminating powers, will be appreciated, when it is considered that with many of them, and those of the denser kind, it is quite saturated. On distilling a portion of liquid, which had condensed in the pipes leading to an oil gas gasometer, and given to me by Mr. Hennel, of the Apothecaries' Hall, I found it to contain portions of the bi-carburet of hydrogen. It was detected by submitting the small quantity of liquid which distilled over before 190° to a cold of 0°, when the substance crystallized from the solution. It is evident, therefore, that the gas from which it was deposited must have been saturated with it. On distilling a portion of recent coal gas tar, as was expected, none could be detected in it, but the action of sulphuric acid is sufficient to show the existence of some of these bodies in the coal gas itself.

With respect to the probable uses of the fluid from compressed oil gas, it is evident in the first place, that being thus volatile, it will, if introduced into gas which burns with a pale flame, give

such quantity of vapour as to make it brightly illuminating; and even the vapour of those portions which require temperatures of 170°, 180°, or higher for their ebullition, is so dense as to be fully sufficient for this purpose in small quantities. A taper was burnt out in a jar of common air over water; a portion of fluid boiling at 190° was thrown up into it, and agitated; the mixture then burnt from a large aperture with the bright flame and appearance of oil gas, though of course many times the quantity that would have been required of oil gas for the same light was consumed: at the same time there was no mixture of blueness with the flame, whether it were large or small. Mr. Gordon has I understand proposed using it in this manner.

The fluid is also an excellent solvent of caoutchouc, surpassing every other substance in this quality. It has already been applied to this purpose.

It will answer all the purposes to which the essential oils are applied as solvents, as in varnishes, &c. and in some cases where volatility is required, when rectified it will far surpass them.

It is possible that, at some future time, when we better understand the minute changes which take place during the decomposition of oil, fat, and other substances by heat, and have more command of the process, that this substance, among others, may furnish the fuel for a lamp, which remaining a fluid at the pressure of two or three atmospheres, but becoming a vapour at less pressure, shall possess all the advantages of a gas lamp, without involving the necessity of high pressure.

Article VI.

Experiments on Anthracite, Plumbago, &c. By Lardner Vanuxem.* (Communicated by the Author.)

These experiments were undertaken with a view to determine whether the globules obtained by Prof. Silliman, from the above substances, were owing to a fusion of their carbon, or merely to the impurities or foreign matter contained within them. They were long delayed by my waiting for some sheet zinc necessary to repair a Deflagrator intended to be used for the purpose of obtaining the globules; but this not arriving, I resolved to avail myself of the suggestion of Professor Silliman, namely, that of using the compound blowpipe, which answered perfectly well. In the experiments with the blowpipe, the substances were placed upon platina foil, spread upon a lump of magnesia; the size of the pieces subjected to its action was

* From the Journal of the Academy of Natural Sciences of Philadelphia.

about half an inch in diameter, and one-fourth of an inch in thickness. The light in the greater number of instances was so intense that I found it necessary to use double green glasses.

The mode pursued in the analysis of anthracite and plumbago was as follows. The presence of water was ascertained by heating a few small pieces of the substance in a glass tube, closed at one end; and the quantity of water by heating a given portion in a covered platina crucible. Another portion was pulverized in an agate mortar; then a given weight of it was put into a platina crucible, and kept without its cover at a red heat in a small French furnace, until the whole of the carbon was consumed; the residue was then boiled in water for an alkali; after which operation it was heated with caustic potash in a silver crucible: when the fusion of the mass was completed, water was added, and the whole then dissolved with nitro-muriatic acid. By evaporating the liquor to dryness, and adding acidulated water and filtering, the silex was obtained. To the liquor from this operation, ammonia in excess was added, and by this agent, the iron, manganese, and alumine contained in the liquor, were precipitated; the latter was separated from the two former by caustic potash. No attempt was made to ascertain the relative proportions of iron and manganese; this knowledge not being considered important. The presence of manganese was evidenced by the green colour of the alkaline fusion; and a rose colour when acid was added to the liquor. No allowance was made for the difference in the degree of oxidation of the iron and manganese in the substances used, and the products obtained, as the amount was less than one per cent. where most abundant.

The first experiments made with the globules were with potash, and with carbonate of soda on silver, and on platina foil; with these agents I could not produce much effect, but by using a small quantity of carbonate of lime, carbonate of soda and borax, on platina foil; their fusion, whether they were coloured or colourless, opaque or transparent, was effected in a few minutes.

Exper. 1.—A piece of the purest anthracite of Lehigh, subjected to the blowpipe, presented numerous small white globules; few were tinged with violet, and two or three were blackish; the globules did not readily unite with one another; however, by long continued heat, some of the globules were obtained of the size of the head of a small pin; the greater number of them were but feebly translucent, and could be broken by a moderate force; others, though few in number, were transparent, hard, and not so brittle. The white globules were not magnetic, except when dark spots were present; the blackish ones were magnetic, and like the whole of them could be fractured by pressure. The surface of the mass whitened, as

observed in the ordinary combustion of this coal, and presented veins or layers of the matter of the white globules; showing that the impurities of the coal were not regularly intermixed with its carbon, or, upon the supposition of its being fused carbon, that its production was extremely irregular.

With the flux before mentioned, the different kinds of globules were melted without difficulty. By heating a centigramme and a half of the globules in powder for a long time with caustic potash, about three-fourths of a centigramme of silex was obtained. It manifested itself by its gelatinous appearance before the water was driven off.

The result of the analysis of this anthracite was,

Carbon			90·1
Water*			6·6
Residue by incineration, of a dirty white colour,	3·3 consisting of	Silex	1·2
		Alumine	1·1
		Oxides of iron and mangan.	0·2
		Loss	0·8
			100·0

Exper. 2.—The anthracite of Rhode Island, by the action of the blowpipe, presented a brownish appearance after cooling (owing to manganese). The surface exhibited numerous globules, larger than those of the Lehigh; some of them were transparent, colourless, and very brilliant by reflected light; others, and the most abundant, were black and opaque, and were strongly attracted by the magnet; a few were coloured white and black in spots; the white spots resembling enamel. The surface of the mass presented minute veins similar to those of the Lehigh.

Some of the black globules were heated for a long time on platina foil with carbonate of soda; the mass was yellowish, but became black when immersed in water. By heating and dipping into water several times, the globules whitened; I could not effect their fusion in this way, but with the compound flux they readily fused. With this flux the different kinds were tried, and with the same effect.

The analysis of this anthracite from Rhode Island gave,

* It is rather singular that so great a quantity of water as is contained in anthracite, should heretofore have escaped notice. It is my intention to examine all the different kinds of coal to ascertain if this fact be general.

Carbon			90·03
Water			4·90
Residue by incineration, which was of a light brick red	5·07 consisting of	Silex	2·14
		Oxides of iron and manganese	2·50
		Loss	0·43
			100·00

Another specimen from the same locality, whose colour was a little different, being of a deeper black, and which was not tried with the blowpipe, gave,

Carbon			77·70
Water			6·70
Residue by incineration, colour the same as the former	15·60 consisting of	Silex	8·50
		Oxides of iron and manganese	7·10
		Alumine	Trace
			100·00

Exper. 3.—A specimen of plumbago from Borrowdale, of great purity, as judged by its external characters and mechanical properties, was subjected to the blowpipe; the globules began to form immediately and in great number, attended occasionally by scintillations, owing to the combustion of iron; the globules were small; the greater part of them were black, opaque, and of great lustre; others were dull, of a brownish colour, and feebly translucent; almost all of them were attracted by the magnet. The surface of the heated part of the plumbago was brownish.

The globules, though acted upon with great difficulty by soda, and by potash, readily yielded to the compound flux, and formed a limpid yellowish glass. A large globule, by repeatedly heating it with carbonate of soda, and plunging it into water, became rough, and finally opened in the centre; it then dissolved in nitro-muriatic acid. By evaporating the liquor to dryness, the yellow colour of the iron was very manifest; acidulated water took it up, leaving a white substance like silex, floating in the liquor.

The analysis of this plumbago gave,

Carbon			88·37
Water			1·23
Residue by incineration, colour, yellowish brick red.	10·4 consisting of	Silex	5·10
		Alumine	1·00
		Oxides of iron and manganese	3·60
		Loss	0·70
			100·00

Exper. 4.—An impure specimen of plumbago from the same locality gave numerous and large globules; some of the size of small shot; they readily formed; the majority of them were translucent, shining, and of a light greenish-yellow, others were dark coloured; and some of them were dull externally. The dark globules, as well as the surface of the mass of plumbago exposed to the flame, was attracted by the magnet; some of the light coloured ones were affected by the magnet, but only at the point where they had been attached to their support, owing to particles of the support adhering to them. During the combustion of the plumbago, there were occasionally scintillations; the heated surface of the mass was brownish.

A large globule of the lightest colour and magnetic only at one point, melted with ease when the compound flux was used; it formed a transparent mass when hot, and opaque and milky when cold. The black ones with the same flux were also fused; they were brownish when hot, and greenish when cold. They were acted upon with great difficulty by caustic potash, and by carbonate of soda.

The analysis of this plumbago gave,

Carbon			61·27
Water			5.33
Residue by incineration, colour of a dirty yellowish red	33·4 consisting of	Silex	10·10
		Alumine	3·20
		Oxide of iron and manganese	20·00
		Loss	0·10
			100·00

Exper. 5.—A specimen of plumbago remarkably pure, from near Bustletown, Penn. was tried with the blowpipe. The globules were formed with difficulty, probably owing to its foliated texture, the fused parts spreading over the surface. The colour in places was white and translucent; in others so dark as to be almost black.

With the flux before mentioned, the fused matter was reduced to a transparent glass.

The analysis of this plumbago gave,

Carbon			94·40
Water			0·60
Residue by incineration, colour light brick red	5·0 consisting of	Silex	2·60
		Oxides of iron and manganese	1·40
		Loss	1·00
			100·00

Similar experiments were made with plumbago from several other localities, the results of which were no wise different, and therefore need no further mention.

Exper. 6.—A piece of charred mahogany, during its combustion by the compound blowpipe, presented numerous small imperfect globules, owing to the force of the flame, which dissipated their support before they had time to form or to accumulate to any considerable size; many of them adhered together, ramifying like flos feri, which they resembled; they were collected by placing a dish under their support. By the compound flux, they readily fused into a transparent glass.

Exper. 7.—A quantity of lamp-black was pressed into a mould with great force, and made to assume the form of a cylinder of about three-fourths of an inch in diameter, and half an inch in thickness; it weighed seven grammes. This cylinder of lamp-black was subjected to the blowpipe. It wasted away gradually without forming any globules or fused matter, visible to the naked eye or to the microscope. The heat was equally as intense in this experiment as in all the other instances, and no condition was wanting to produce the same effects, except the difference of composition. After burning the lamp-black for as long a time as was thought necessary to make the experiment a fair one, it was again weighed, and found to have lost four grammes, $\frac{42}{100}$, for it weighed but two grammes, $\frac{58}{100}$.

Five grammes of the same lamp-black heated in an open platina crucible, left after its incineration one centigramme of white ashes, equal to $\frac{1}{500}$ of the mass.

From the analyses of the substances used by Prof. Silliman, from which the globules were obtained, it appears that they all contain foreign matter, as silex, iron, manganese, and some of them also alumine; that when lamp-black was used which contained but $\frac{1}{500}$ of fixed impurities, no distinct globule or melted matter was formed; although the heat was sufficiently great, and the combustion slow enough to admit of the forming of globules, if their production was owing to the fusion of carbon, and not to extraneous matter. From my own experiments I always found that the more impure the substance was, the more numerous and the larger were the globules produced.

All the globules from the different kinds of substances used, were readily fused by the compound flux, and underwent little change when it was not used; although the heat was, in this case, of longer continuance. Matter similar to the impurities discovered in the substances used was detected in them.

From these facts it would appear, that the globules produced from the combustible substances operated upon, did not arise from the fusion of their carbon, since they can otherwise be accounted for; particularly as no experiment has been made which unequivocally leads to that conclusion. The experiment

upon which Prof. Silliman relies, as a proof of the globules being fused carbon, is one which is not satisfactory to me; if it had been, it would have given me great pleasure; for no one, I trust, feels more interested in the scientific prosperity of his country than I do; and if Prof. Silliman were right, it would indeed be a triumph for America.

The experiment just alluded to (see vol. vi. p. 347, Journal of Science), is the heating some of the coloured globules in oxygen gas by the solar rays, with a lens. The following is an extract from the papers.

"To detach any portion of unmelted plumbago which might adhere to them, I carefully rubbed them between my thumb and finger, in the palm of my hand. Although they were in the focus for nearly half an hour, they did not melt, disappear, or alter their form; it appeared, however, on examining the gas, that they had given up a part of their substance to the oxygen, for carbonic acid was formed which gave a decided precipitate with lime-water."

That this experiment is equivocal appears certain, as particles of the support might have been attached to the globules; for, from my own observations, I found that in a great number of instances, some of the white globules at the point of junction with their support, had small dark particles attached to them, and when the surface from which they were detached was magnetic, they were attracted by the magnet when it was presented to those parts; I could not disengage those particles by rubbing the globules with my fingers against one another. It is very evident that as the globules underwent no change (unless a reduction of volume, which is not mentioned), as the description clearly shows, the carbonic acid obtained, might have been produced by the combustion of portions of the support adhering to them externally, and penetrating them to a certain extent.

In the experiment detailed in vol. v. p. 363, of the same Journal, the carbonic acid found, probably had a similar origin, and the disappearance of the globules may have been owing to their incorporating themselves with the piece of brick upon which they were placed, as the brick was vitrified at the point where they were placed.

Prof. Silliman seems disposed to lay great stress on the loss in my examination of the globule, sent by Dr. Macneven. I thought I had well accounted for it, as the particle was small, action violent, and I merely wished to show chemically the presence of iron. I could not, for one moment, entertain the idea that carbon existed in it in any notable proportion; for I know of no combination of iron and carbon at common temperature which could give a product possessed of the malleability and toughness which the globule possessed.

I was sorry to observe that Professor Silliman in his reply to

my paper seems offended that I did not notice his communications upon the subject of these globules, particularly as the discovery was his, and was justly entitled to such consideration. My silence certainly appeared uncourtly, but it was not owing to ignorance of his labours, or a want of regard to him personally, or as a chemist; Prof. Silliman's merit is too well known to be affected by me.

Article VII.

On the Combustion of Compressed Gas; being part of a paper read before the Literary and Philosophical Society of Manchester. By Mr. J. Davies, MWS. &c. &c. Lecturer on Chemistry, &c. (Communicated by the Author.)

In making, upwards of twelve months ago, some experiments upon the combustion of compressed gas, I accidentally observed a fact which is, I think, of rather a singular nature.

When the aperture of the burner is, in this case, too large, the flame cannot be maintained, being blown away by the rapid current of the gas. When it is rather small, the flame is under the best circumstances. If the aperture be further enlarged without being carried to the extent at which the combustion is extinguished, the flame will then be blue, noisy, and agitated, affording very little light. But I found, to my great surprise, that if, when the flame was in this last state, the vessel of the gas was inverted, the flame was instantly changed, and instead of being as I have just stated, it was steady, silent, and powerful. I have repeated the experiment frequently, and with different vessels. In every instance the result has been precisely the same.

It became interesting to inquire into the cause of the phænomenon. I submit with deference the only explanation which I have been able to discover.

The gas, rarefied by heat, being lighter than the atmosphere, has a tendency to move in the direction of the flame when the vessel is held upright. In this case, therefore, it moves with greater impetuosity than it could were the burner in any other position. On the contrary, when the flame is directed downwards, it has a tendency to return upon itself. Thus the ascent of the gas is promoted, and the descent retarded, by the agency of the atmosphere; for the gas being rendered lighter in the way just mentioned, has a tendency to rise in the air on the same principle that a cork rises in water, and its descent is in like manner resisted. The fact might, perhaps, be better illustrated by conceiving air to be forced through water. If the air be urged from the bottom of the vessel, it readily moves by

reason of its great levity in the required direction; but if it be forcibly impelled downwards from the surface, as from the extremity of a condensing syringe, it can only be driven to a short distance, and it is then forced back towards the pipe. This case appears to me to be analogous to that of the gas, which, if I am not mistaken, it serves to illustrate and explain. The upright position of the vessel admits, in the case referred to, of the escape of some of the gas unburnt; but when the burner is inverted, the flame, for reasons already assigned, returns upon the stream of gas, and the combustion, which was before imperfect, is then complete.

How far the fact may be susceptible of a practical application, I am not at present prepared to offer an opinion; but the consumption of the gas is, by this mode of burning, very considerable, and I have not yet been able to determine that there is in the combustion of gas under the ordinary pressure, any increase of illuminating power obtained by inverting the burner.

Article VIII.

The Hedgehog-Ray—a species of Fish taken occasionally near New York, in the Atlantic Ocean, and now, as is believed, for the first time described. By Samuel L. Mitchill, M. and LL. D. &c.*

The fish brought me this morning by Capt. Enos Woodruff, was taken by him with a hook and line, in the sea, off Barnegat, where the water was seven fathoms deep. It had been wounded so slightly that he kept it alive for several days, and he supposed it might have been living yet, had it not perished in consequence of the highly electrical state of the atmosphere during the late shower, accompanied by remarkably bright lightning, and loud thunder. His belief is, according to the opinion prevailing among fishermen, that the thunder killed the fish.

The animal undoubtedly belongs to the great family of Raja, which comprehends the Rays, Skates, Torpedoes, and most of the other horizontally flat fishes not appertaining to the *Pleuronectes*, or flounder tribe.

When drawn from its element, it had the appearance, for some minutes, while its vital energy remained, and it was yet pendant from the hook, of a hedgehog: that is to say, a contraction of the muscles had taken place, by which the approximated margin, or circumference, from the several parts, resembled a bowl, or basket, of which the belly was the inner,

* American Journal of Science.

and the back the outer side. The tail, at the time, was incurvated so much as to enter the mouth, or project beyond it. When in this posture, the fish seemed capable of presenting the globular or spherical form of the back, with its armature and prickles, to its enemies or pursuers. For, even when held in the air, its rotundity remained until the muscles were relaxed by death; and, even then, after animation was extinct, there was a curvature of the rim, or periphery, showing its tendency to a concave figure. The only other individual of the species I ever saw, was one that was catched, in my presence, on board the boat that went to the fishing banks, south-east of Sandy-Hook, on the 23d July, 1822. I examined it while alive, and immediately on being raised from the depth of five fathoms. I then named it

RAJA ERINACEUS,

with this specific character: " having a tail bearing two dorsal fins, with the vestige of a third at the extremity; thickly acuelated on the sides, though destitute of the spines called stings; having a pale brown prickly skin, over which dark brown spots are distributed; and having also a patch of about 20 spines on each wing, or flap, which, while the wings or flaps are extended, and lie flat, are concealed or covered by the skin; but, when the wings or flaps are contracted, come forth and are erected like the claws of a cat, when they are capable of arresting or tearing soft objects presented to them."

The length of the specimen now before me is 17 inches, and the breadth 9½ inches. The head is roundish, though ending in something like a pointed snout. The cheeks (if they may be so called) are parting projections, of a curved form, on the sides of the snout, and are laterally anterior to the eyes. The pectoral fins (wings or flaps) are circular or roundish, and, viewed in connexion, present a sort of elliptical figure. The ventral fins have three little elevations or protuberances backward, that might almost be called digitations, as there are traces, within the common integuments, of concealed fingers. The anal fins have no striking peculiarities. Near the base of these, and under the tail, the two appendages, peculiar to these creatures, proceed obliquely to the length of five inches.

The whole body is so semi-diaphanous that the bones can be discerned on holding it up between the eye and the light. This quality distinguishes the marginal parts of the flaps particularly, and yet more distinctly characterizes the snout.

Tail thick and stout, like that of the skate; and, measured from the base to the ventrals, nine inches long. Toward the extremity, it supports two fins, which are faintly radiated. The foremost of these is jagged behind with several slits or notches: the hindermost has no such divisions. There is a trace of a

third fin, near the very end of the tail, in the form of a neat film.

Skin slimy and scaleless. It is beset with prickles in spots or patches. There is a patch in front of each eye, reaching along the inner orbit, and likewise occupying the space between the eyes. Two lines of spines proceed, one from each ocular patch, to the tip of the snout, where they join, in the form of the letter V inverted. The cheeks, or lateral pouches, are covered with prickles, so as to bear some resemblance to whiskers.

Behind the eyes, and on the back part of the head, there is a patch of prickles, in the shape of an equilateral triangle, with one of its sides backwards, and an angle forward.

On each wing, or flap, is a patch of catspaw prickles, of the retractile quality, mentioned in the definition. From the moustaches, the skin of the flaps, along the edge, and for a small distance beyond, is roughtened by a set of more minute prickles.

Along each side of the back is a row of stiff and short spines, proceeding towards the tail; and smaller ones near them, with a rather irregular distribution. On the tail they are much more numerous, distinct, and strong; distinguishable in two main rows, or lines, with a smooth scaleless and spineless stripe between them, reaching to the dorsals. The lower side of the tail, and the whole belly, are quite smooth. There is a trifling roughness on a patch of each caudal appendage.

Eyes half covered and elegantly curtained. Behind them open and ample orifices, or ears. Nostrils distinct, and connected with the mouth, through fissures, to the upper lip. Teeth, in both jaws, associated, compact, and sharp-pointed.

The lower or belly side of this fish exhibits a bending, or inflexion of the margin, all the way round to the ventral fins, of such a kind that when, even after death, it lies upon its back, there is a rising, or rim, like that of a cup or basin, capable of preventing the escape of water.

Article IX.

On Climate, considered with regard to Horticulture. By John Frederic Daniell, Esq. FRS. &c.* (Communicated by the Author.)

The following observations were committed to paper, and submitted to the consideration of the Horticultural Society, at the particular request of their Secretary. The author would

* From the Horticultural Transactions.

scarcely have thought them novel or important enough for such a destination, but he defers to his judgment, and shall at all events have had the pleasure of complying with his wishes.

Horticulture differs from agriculture in one very material respect. The latter has for its object the fertilization of the soil by manures, and the different processes of cultivation, in the manner best adapted to the peculiarities of any given climate: it concerns itself only with the growth and nourishment of such plants as are indigenous, or, by a long course of treatment, have become inured to the vicissitudes of weather incidental to a particular latitude. The former occupies a much wider field of research; it not only seeks to be conversant with the constitution of soils, but as it aspires to the preservation and propagation of exotic vegetation, it necessarily embraces the consideration of varieties of climate: and it labours, by art, to assimilate the confined space of its operations to that constitution of atmosphere which is most congenial to its charge, or to protect them at different periods of their growth from sudden changes of weather which would be detrimental to their health. Experience has anticipated theoretical knowledge in suggesting various artifices, by which these ends may be effected; a connected view of which has never, I believe, been attempted; but may prove to be not without interest and utility. The suggestions of experience may probably enlarge the conclusions of theory, while it is not impossible that the improved state of the latter may be found to furnish some assistance to the former.

The science of Horticulture, with regard to climate, will be best considered in two divisions: the first comprises the methods of mitigating the extremes, or exalting the energies, of the natural climate in the open air; the second embraces the more difficult means of composing and maintaining a confined atmosphere, whose properties may assimilate with those of the natural atmosphere in intertropical latitudes. I shall commence my observations with the former.

The basis of the atmosphere has been proved to be of the same chemical composition in all the regions of the globe. All the varieties of climate will therefore be found to depend upon the modifications impressed upon it by light, heat, and moisture, and over these, art has obtained, even in the open air, a greater influence than at first sight would appear to be possible. By judicious management, the climate of our gardens is rendered congenial to the luxurious productions of more favoured regions, and flowers and fruits from the confines of the tropics, flourishing in the open air, daily prove the triumphs of knowledge and industry.

For the complete understanding of the subject in all its bearings, and to enable us to derive all the practical advantages which such an understanding would certainly afford, it would

be necessary to have a full knowledge of the peculiarities of the climate of every region of the earth, a knowledge which we are very far from yet possessing; but to which, rapid advances are daily making. But above all, it seems necessary that we should understand the atmospheric variations of our own situation. These, though not constituting the greatest range with which we are acquainted, are great, and oftentimes sudden. The range of the thermometer in the shade is from 0° to 90° of Fahrenheit's scale; but under favourable circumstances the heat of the sun's rays reaches 135°, the changes of moisture extend from 1·000, or saturation, to ·389.* Now the great object of the Horticulturist is to stretch, as it were, his climate to the south, where these extremes of drought and cold never occur; and not only to guard against the injurious effects of the ultimate severity of weather, but to ward off the sudden changes which are liable to recur in the different seasons of the year. To enable us to understand the methods of effecting this end, it will be necessary to consider the means by which these changes are brought about in the general course of nature. The principal of these will be found to be, wind and radiation.

The amount of evaporation from the soil, and of exhalation from the foliage of the vegetable kingdom, depends upon two circumstances, the saturation of the air with moisture, and the velocity of its motion. They are in inverse proportion to the former, and in direct proportion to the latter.

When the air is dry, vapour ascends in it with great rapidity from every surface capable of affording it, and the energy of this action is greatly promoted by wind, which removes it from the exhaling body as fast as it is formed, and prevents that accumulation which would otherwise arrest the process.

Over the state of saturation, the Horticulturist has little or no control in the open air, but over its velocity he has some command. He can break the force of the blast by artificial means, such as walls, palings, hedges, or other screens; or he may find natural shelter in situations upon the acclivities of hills. Excessive exhalation is very injurious to many of the processes of

* The *Dew-point* (a term which will often occur in the course of this paper) is the degree of temperature at which the moisture of the atmosphere would begin to precipitate, and may readily be ascertained at all seasons by means of the hygrometer. The natural scale of the hygrometer is included between the points of perfect dryness and perfect moisture: the latter, of course, being that state of the atmosphere at which the *dew-point* coincides with the temperature of the air. The intermediate degrees may be ascertained by dividing the elasticity of vapour at the temperature of the dew-point by the elasticity at the temperature of the air: the quotient will express the proportion of moisture actually existing, to the quantity which would be required for saturation; for, calling the term of saturation 1·000, as the elasticity of vapour at the temperature of the air is to the elasticity of vapour at the temperature of the dew-point, so is the term of saturation to the actual degree of moisture. The necessary tables for facilitating this calculation, and more detailed explanations than it is possible to comprise in a note, may be had, with the hygrometer, at Mr. Newman's in Lisle-street, or may be found in the author's *Meteorological Essays*.

vegetation, and no small proportion of what is commonly called *blight* may be attributed to this cause. Evaporation increases in a prodigiously rapid ratio with the velocity of the wind, and any thing which retards the motion of the latter is very efficacious in diminishing the amount of the former; the same surface, which in a calm state of the air would exhale 100 parts of moisture, would yield 125 in a moderate breeze, and 150 in a high wind. The dryness of the atmosphere in spring renders the effect most injurious to the tender shoots of this season of the year, and the easterly winds especially are most to be opposed in their course. The moisture of the air flowing from any point between NE and SE inclusive is to that of the air from the other quarter of the compass, in the proportion of 814 to 907 upon an average of the whole year: and it is no uncommon thing in spring for the dew-point to be more than 20 degrees below the temperature of the atmosphere in the shade, and I have even seen the difference amount to 30 degrees. The effect of such a degree of dryness is parching in the extreme, and if accompanied with wind is destructive to the blossoms of tender plants. The use of high walls, especially upon the northern and eastern sides of a garden, in checking this evil, cannot be doubtful, and in the case of tender fruit trees, such screens should not be too far apart.

And here theory would suggest another precaution, which I believe has never yet been adopted, but which would be well worthy of a trial. When trees are trained upon a wall with a southern aspect, they have the advantage of a greatly exalted temperature, but this temperature in spring differs from the warmth of a more advanced period of the year, or of a more southern climate, in not being accompanied by an increase of moisture. In the extremely dry state of the atmosphere to which I am now alluding, the enormous exhalation from the blossoms of tender fruit trees, which must thus be induced, cannot fail of being extremely detrimental; the effect of shading the plants from the direct rays of the sun should therefore be ascertained. The state of the weather to which I refer often occurs in April, May, and June, but seldom lasts many hours. Great mischief, however, may arise in a very small interval of time, and the disadvantage of a partial loss of light cannot be put in comparison with the probable effect which I have pointed out.

During the time in which I kept a register of the weather, I have seen in the month of May the thermometer in the sun at 101°, while the dew-point was only 34°, the state of saturation of the air, upon a south wall, consequently only amounted to 120, a state of dryness which is certainly not surpassed by an African Harmattan. The shelter of a mat on such occasions

would often prevent the sudden injury which so frequently arises at this period of the year.

Some of the present practices of gardening are founded upon experience of similar effects, and it is well known that cuttings of plants succeed best in a border with a northern aspect protected from the wind; or if otherwise situated, they require to be screened from the force of the noon-day sun. If these precautions be unattended to, they speedily droop and die. For the same reason, the autumn is selected for placing them in the ground, as well as for transplanting trees; the atmosphere at that season being saturated with moisture is not found to exhaust the plant before it has become rooted in the soil.

Over the absolute state of vapour in the air we are wholly powerless, and by no system of watering can we affect the dew-point in the free atmosphere. This is determined in the upper regions; it is only therefore by these indirect methods, and by the selection of proper seasons, that we can preserve the more tender shoots of the vegetable kingdom from the injurious effects of excessive exhalation.

Radiation, the second cause which I have mentioned as producing a sudden and injurious influence upon the tender products of the garden, is one that has been little understood, till of late years, by the natural philosopher; and even to this day has not been rendered familiar to the practical gardener; who, although he has been taught by experience to guard against some of its effects, is totally unacquainted with the theory of his practice. Dr. Wells, to whose admirable "Essay upon Dew," we are so much indebted for our present knowledge upon this important subject, thus candidly remarks upon this anticipation of science: "I had often, in the pride of half knowledge, smiled at the means frequently employed by gardeners to protect tender plants from cold, as it appeared to me impossible that a thin mat or any such flimsy substance could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But when I had learned that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice which I had before deemed useless."

The power of emitting heat in straight lines in every direction, independently of contact, may be regarded as a property common to all matter, but differing in degree in different kinds of matter. Co-existing with it, in the same degrees, may be regarded the power of absorbing heat so emitted from other bodies. Polished metals, and the fibres of vegetables, may be considered as placed at the two extremities of the scale upon which these properties in different substances may be measured.

If a body be so situated that it may receive just as much radiant heat as itself projects, its temperature remains the same; if the surrounding bodies emit heat of greater intensity than the same body, its temperature rises, till the quantity which it receives exactly balances its expenditure, at which point it again becomes stationary; and if the power of radiation be exerted under circumstances which prevent a return, the temperature of the body declines. Thus, if a thermometer be placed in the focus of a concave metallic mirror, and turned towards any clear portion of the sky, at any period of the day, it will fall many degrees below the temperature of another thermometer placed near it, out of the mirror; the power of radiation is exerted in both thermometers, but to the first all return of radiant heat is cut off, while the other receives as much from the surrounding bodies as itself projects. This interchange amongst bodies takes place in transparent *media* as well as in *vacuo;* but in the former case the effect is modified by the equalizing power of the medium.

Any portion of the surface of the globe which is fully turned towards the sun receives more radiant heat than it projects, and becomes heated; but when, by the revolution of the axis this portion is turned from the source of heat, the radiation into space still continues, and being uncompensated, the temperature declines. In consequence of the different degrees in which different bodies possess this power of radiation, two contiguous portions of the system of the earth will become of different temperatures, and if on a clear night we place a thermometer upon a grass-plat, and another upon a gravel walk or the bare soil, we shall find the temperature of the former many degrees below that of the latter. The fibrous texture of the grass is favourable to the emission of the heat, but the dense surfaces of the gravel seem to retain and fix it. But this unequal effect will only be perceived when the atmosphere is unclouded, and a free passage is open into space; for even a light mist will arrest the radiant matter in its course, and return as much to the radiating body as it emits. The intervention of more substantial obstacles will of course equally prevent the result, and the balance of temperature will not be disturbed in any substance which is not placed in the clear aspect of the sky. A portion of a grass-plat under the protection of a tree or hedge, will generally be found, on a clear night, to be eight or ten degrees warmer than surrounding unsheltered parts, and it is well known to gardeners that less dew and frost are to be found in such situations than in those which are wholly exposed.

There are many independent circumstances which modify the effects of this action, such as the state of the radiating body, its power of conducting heat, &c. If, for instance, the body be in a liquid or aeriform state, although the process may go on freely, as in water, the cold produced by it will not accumulate upon

the surface, but will be dispersed by known laws throughout the mass; and if a solid body be a good radiator but a bad conductor of heat, the frigorific effect will be condensed upon the face which is exposed. So upon the surface of the earth absolute stillness of the atmosphere is necessary for the accumulation of cold upon the radiating body; for if the air be in motion, it disperses and equalizes the effect, with a rapidity proportioned to its velocity.

It is upon these principles that Dr. Wells has satisfactorily explained all the phenomena connected with dew or hoar frost. This deposition of moisture is owing to the cold produced in bodies by radiation, which condenses the atmospheric vapour upon their surfaces. It takes place upon vegetables, but not upon the naked soil. The fibres of short grass are particularly favourable to its formation. It is not produced either in cloudy or in windy weather, or in situations which are not perfectly open to the sky. It is never formed upon the good conducting surfaces of metals, but is rapidly deposited upon the badly conducting surfaces of filamentous bodies, such as cotton, wool, &c.

In remarking that dew is never formed upon metals, it is necessary to distinguish a secondary effect which often causes a deposition of moisture upon every kind of surface indiscriminately. The cold which is produced upon the surface of the radiating body is communicated by slow degrees to the surrounding atmosphere, and if the effect be great and of sufficient continuance, moisture is not only deposited upon the solid body, but is precipitated in the air itself; from which it slowly subsides, and settles upon every thing within its range.

The formation of dew is one of the circumstances which modify and check the refrigerating effect of radiation; for as the vapour is condensed, it gives out the latent heat with which it was combined in its elastic form, and thus, no doubt, prevents an excess of depression which might in many cases prove injurious to vegetation. A compensating arrangement is thus established, which, while it produces all the advantages of this gentle effusion of moisture, guards against the injurious concentration of the cause by which it is produced.

The effects of radiation come under the consideration of the Horticulturist in two points of view: the first regards the primary influence upon vegetables, exposed to it; the second the modifications produced by it upon the atmosphere of particular situations. To vegetables growing in the climates for which they were originally designed by nature, there can be no doubt that the action of radiation is particularly beneficial, from the deposition of moisture which it determines upon their foliage; but to tender plants artificially trained to resist the rigours of an unnatural situation, this extra degree of cold may prove highly prejudicial. It also appears probable from observation, that the

intensity of this action increases with the distance from the equator to the poles; as the lowest depression of the thermometer which has been registered between the tropics, from this cause, is 12°, whereas in the latitude of London, it not unfrequently amounts to 17°. But however this may be, it is certain that vegetation in this country is liable to be affected at night from the influence of radiation by a temperature below the freezing point of water, ten months in the year; and even in the two months, July and August, which are the only exceptions, a thermometer covered with wool will sometimes fall to 35°. It is, however, only low vegetation upon the ground which is exposed to the full rigour of this effect. In such a situation the air which is cooled by the process, lies upon the surface of the plants, and from its weight cannot make its escape; but from the foliage of a tree or shrub, it glides off and settles upon the ground.

Any thing which obstructs the free aspect of the sky arrests in proportion the progress of this refrigeration, and the slightest covering of cloth or matting annihilates it altogether. Trees trained upon a wall or paling, or plants sown under their protection, are at once cut off from a large portion of this evil; and are still further protected, if within a moderate distance of another opposing screen. The most perfect combination for the growth of exotic fruits in the open air would be a number of parallel walls within a short distance of one another, facing the south-east quarter of the heavens; the spaces between each should be gravelled, except a narrow border on each side, which should be kept free from weeds and other short vegetables. On the southern sides of these walls, peaches, nectarines, figs, &c. might be trained to advantage, and on their northern sides many hardier kinds of fruit would be very advantageously situated. Tender exotic trees would thus derive all the benefit of the early morning sun, which would at the earliest moment dissipate the greatest accumulation of cold which immediately precedes its rise, and the injurious influence of nocturnal radiation would be almost entirely prevented. Upon trees so trained, the absolute perpendicular impression could have little effect, and this little might even be prevented by a moderate coping.

Mats or canvass, upon rollers to draw down occasionally in front of the trees, at the distance of a foot or two from their foliage, would, I have no doubt, be a great advantage in certain dry states of the atmosphere before alluded to, and in the case of walls which are not opposed to others, would be a good substitute for the protection of the latter.

Experience has taught gardeners the advantages of warding off the effects of frost from tender vegetables, by loose straw or other litter, but the system of matting does not appear to be

carried to that extent which its simplicity and efficacy would suggest. Neither does the manner of fixing the screen exhibit a proper acquaintance with the principle upon which it is resorted to: it is generally bound tight round the tree which it is required to protect, or nailed in close contact with its foliage. Now it should be borne in mind that the radiation is only tranferred from the tree to the mat, and the cold of the latter will be conducted to the former in every point where it touches. Contact should therefore be prevented by hoops or other means properly applied, and the stratum of air which is enclosed will, by its low conducting power, effectually secure the plant. With their foliage thus protected, and their roots well covered with litter, many evergreens might doubtless be brought to survive the rigour of our winters, which are now confined to the stunted growth of the green house and conservatory.

The secondary effect which radiation has upon the climate of particular situations, is a point which is less frequently considered than the primary one which we have been investigating, but which requires perhaps still more attention. The utmost concentration of cold can only take place in a perfectly still atmosphere: a very slight motion of the air is sufficient to disperse it. A low mist is often formed in meadows in particular situations, which is the consequence of the slow extension of this cold in the air, as before described; the agitation of merely walking through this condensation is frequently sufficient to disperse and melt it. A valley surrounded by low hills is more liable to the effects of radiation than the tops and sides of the hills themselves; and it is a well known fact that dew and hoar frost are always more abundant in the former than in the latter situations. It is not meant to include in this observation places surrounded by lofty and precipitous hills which obstruct the aspect of the sky, for in such the contrary effect would be produced. Gentle slopes which break the undulations of the air without naturally circumscribing the heavens, are most efficient in promoting this action, and it is worthy of remark and consideration, that by walls and other fences, we may artificially combine circumstances which may produce the same injurious effect.

But the influence of hills upon the nightly temperature of the valleys which they surround is not confined to this insulation; radiation goes on upon their declivities, and the air which is condensed by the cold rolls down and lodges at their feet.

Their sides are thus protected from the chill, and a double portion falls upon, what many are apt to consider, the more sheltered situation. Experience amply confirms these theoretical considerations. It is a very old remark, that the injurious effects of cold occur chiefly in hollow places, and that frosts are less severe upon hills than in neighbouring plains. It is

consistent with my own observations that the leaves of the vine, the walnut-tree, and the succulent shoots of dahlias and potatoes, are often destroyed by frost in sheltered valleys, on nights when they are perfectly untouched upon the surrounding eminences; and I have seen a difference of 30 degrees on the same night between two thermometers placed in the two situations, in favour of the latter.

The advantages of placing a garden upon a gentle slope must be hence very apparent: a running stream at its foot would secure the further benefit of a contiguous surface, not liable to refrigeration, and would prevent any injurious stagnation of the air. Few situations are likely to fulfill all the conditions which theory would suggest for the most perfect mitigation of the climate in the open air; but the preceding remarks may not be without their use in pointing out localities, which, with this view, are most to be avoided.

Little is in the power of the Horticulturist to effect in the way of exalting the powers of the climate in the open air; except by choice of situation with regard to the sun and the concentration of its rays upon walls and other screens. The natural reverberation from these and the subjacent soil, is however very effective, and few of the productions of the tropical regions are exposed to a greater heat than a well trained tree upon a wall in summer. Indeed it would appear from experiment, that the power of radiation from the sun, like that of radiation from the earth, increases with the distance from the equator, and there is a greater difference between a thermometer placed in the shade, and another in the solar rays in this country, than in Sierra Leone, or Jamaica. The observations of the President of this Society upon the growth of pine-apples is in exact accordance with this idea, for he has remarked that this species of plant, though extremely patient of a high temperature, is not by any means so patient of the action of very continued bright light as many other plants, and much less so than the fig and orange tree; and he is inclined to think that on this account they may be found to ripen their fruit better in the spring than in the middle of the summer.* This energy of the sun is at times so great that it often becomes necessary to shade delicate flowers from its influence, and I have already pointed out a case in which it would be desirable to try the same precaution with the early blossom of certain fruit trees. The greatest power is put forth in this country in June, while the greatest temperature of the air does not take place till July. The temperature of summer may thus be anticipated a month, in well secured situations.

The greatest disadvantage to which Horticulture is subject in

* See Horticultural Transactions, vol. iv. page 548.

this climate, is the uncertainty of clear weather; a circumstance which art has, of course, no means to control; no artificial warmth is capable of supplying the deficiency when it occurs, and without the solar beams fruits lose their flavour and flowers the brightness of their tints. It has been attempted to communicate warmth to walls by means of fire and flues, but without the assistance of glass no great success has attended the trial.

It is well known that solar heat is absorbed by different substances with various degrees of facility dependant upon their colours, and that black is the most efficacious in this respect. It has therefore been proposed to paint garden walls of this colour; but no great benefit is likely to arise from this suggestion. It is probable that in the spring, when the trees are devoid of foliage, the wood may thus be forced to throw out its blossom somewhat earlier than it otherwise would; but this would be rather a disadvantage, as the flower would become exposed to the vicissitudes of an early spring. It is more desirable to check than to force this delicate and important process of vegetation, as much injury may arise from its premature developement. When the tree has put forth its foliage, the colour of its protecting support can have no influence in any way: the leaves cover the surface and absorb the rays by their own inherent powers. The only known advantage which can be taken of this peculiar power in dark substances, is in the case of covering up fruits, to preserve them from the ravages of flies; grapes which are enclosed in bags of black crape ripen better than those in white; but I believe that it is admitted that neither do so well as those which are freely exposed.

I come now to the consideration of a confined atmosphere, the management of which being entirely dependent upon art, requires in the Horticulturist a more extended acquaintance with the laws of nature, with regard to climate, and greater skill and experience in the application of his means. The plants which require this protection are in the most artificial state which it is possible to conceive; for not only are their stems and foliage subject to the vicissitudes of the air in which they are immersed, but in most cases their roots also. The soil in which they are set to vegetate is generally contained in porous pots of earthen ware, to the interior surface of which the tender fibres quickly penetrate and spread in every direction; they are thus exposed to every change of temperature and humidity, and are liable to great chills from any sudden increase of evaporation. This part of the subject naturally divides itself into two branches. The first regards the treatment of such exotics as are wholly dependent upon the artificial atmosphere of hot-houses: the second refers to the management of those hardier plants which only require to be preserved in green-houses part of the year,

but during the summer months are exposed to the changes of the open air. I shall offer a few remarks first on the atmosphere of a hot-house.

The principal considerations which generally guide the management of gardeners in this delicate department are those of temperature; but there are others regarding moisture which are, I conceive, of at least equal importance. The inhabitants of the hot-house are all natives of the torrid zone, and the climate of this region is not only distinguished by an unvarying high degree of heat, but also by a very vaporous atmosphere. Captain Sabine, in his meteorological researches between the tropics, rarely found, at the hottest period of the day, so great a difference as ten degrees between the temperature of the air and the dew-point; making the degree of saturation about 730, but most frequently 5° or 850°; and the mean saturation of the air could not have exceeded 910. Now I believe that if the hygrometer were consulted, it would be no uncommon thing to find in hot-houses, as at present managed, a difference of 20° between the point of condensation and the air, or a degree of moisture falling short of 500. The danger of over-watering most of the plants, especially at particular periods of their growth, is in general very justly appreciated; and in consequence the earth at their roots is kept in a state comparatively dry; the only supply of moisture being commonly derived from the pots, and the exhalations of the leaves is not enough to saturate the air, and the consequence is a prodigious power of evaporation. This is injurious to the plants in two ways: in the first place, if the pots be at all moist, and not protected by tan or other litter, it produces a considerable degree of cold upon their surface, and communicates a chill to the tender fibres with which they are lined. The danger of such a chill is carefully guarded against in the case of watering, for it is one of the commonest precautions not to use any water of a temperature at all inferior to that of the hot air of the house; inattention to this point is quickly followed by diastrous consequences. The danger is quite as great from a moist flower pot placed in a very dry atmosphere.

The custom of lowering the temperature of fluids in hot climates, by placing them in coolers of wet porous earthen ware, is well known, and the common garden pot is as good a cooler for this purpose as can be made. Under the common circumstances of the atmosphere of a hot-house, a depression of temperature amounting to 15 or 20 degrees, may easily be produced upon such an evaporating surface. But the greatest mischief will arise from the increased exhalations of the plants so circumstanced, and the consequent exhaustion of the powers of vegetation. The flowers of the torrid zone are many of them of a very succulent nature, largely supplied with cuticular pores, and their tender buds are unprovided with those integuments

and other wonderful provisions by which nature guards her first embryo productions in more uncertain climates. Comparatively speaking they shoot naked into the world, and are suited only to that enchanting mildness of the atmosphere, for which the whole system of their organization is adapted. In the tropical climates the sap never ceases to flow, and sudden checks or accelerations of its progress are as injurious to its healthy functions as they are necessary in the plants of more variable climates to the formation of those *hybernacula* which are provided for the preservation of the shoots in the winter season. Some idea may be formed of the prodigiously increased drain upon the functions of a plant arising from an increase of dryness in the air from the following consideration. If we suppose the amount of its perspiration, in a given time, to be 57 grains, the temperature of the air being 75°, and the dew-point 70, or the saturation of the air being 849, the amount would be increased to 120 grains in the same time if the dew-point were to remain stationary, and the temperature were to rise to 80°; or in other words, if the saturation of the air were to fall to 726.

Besides this power of transpiration, the leaves of vegetables exercise also an absorbent function, which must be no less disarranged by any deficiency of moisture. Some plants derive the greatest portion of their nutriment from the vaporous atmosphere, and all are more or less dependent upon the same source. The *Nepenthes distillatoria* lays up a store of water in the cup formed at the end of its leaves, which is probably secreted from the air, and applied to the exigencies of the plant when exposed to drought, and the quantity, which is known to vary in the hothouse, is no doubt connected with the state of moisture of the atmosphere.

These considerations must be sufficient, I imagine, to place in a strong light the necessity of a strict attention to the atmosphere of vapour in our artificial climates, and to enforce as absolute an imitation as possible of the example of nature. The means of effecting this is the next object of our inquiry.

Tropical plants require to be watered at the root with great caution, and it is impossible that a sufficient supply of vapour can be kept up from this source alone. There can however be no difficulty in keeping the floor of the house and the flues continually wet, and an atmosphere of great elasticity may thus be maintained in a way perfectly analogous to the natural process. Where steam is employed as the means of communicating heat, an occasional injection of it into the air may also be had recourse to; but this method would require much attention on the part of the superintendant, whereas the first cannot easily be carried to excess.

It is true that damp air or floating moisture of long continuance would also be detrimental to the health of the plants, for it

is absolutely necessary that the process of transpiration should proceed; but there is no danger that the high temperature of the hot-house should ever attain the point of saturation by spontaneous evaporation. The temperature of the external air will always keep down the force of the vapour; for as in the natural atmosphere the dew-point at the surface of the earth is regulated by the cold of the upper regions, so in a house the point of deposition is governed by the temperature of the glass with which it is in contact. In a well ventilated hot-house, by watering the floor in summer, we may bring the dew-point within four or five degrees of the temperature of the air, and the glass will be perfectly free from moisture; by closing the ventilators we shall probably raise the heat 10 or 15 degrees, but the degree of saturation will remain nearly the same, and a copious dew will quickly form upon the glass, and will shortly run down in streams. A process of distillation is thus established, which prevents the vapour from attaining the full elasticity of the temperature.

This action is beneficial within certain limits, and at particular seasons of the year, but when the external air is very cold, or radiation proceeds very rapidly, it may become excessive and prejudicial. It is a well known fact, but one which I believe has never yet been properly explained, that by attempting to keep up in a hot-house the same degree of heat at night as during the day, the plants become scorched; from what has been premised it will be evident that this is owing to the low temperature of the glass, and the consequent low dew-point in the house, which occasions a degree of dryness which quickly exhausts the juices.

Much of this evil might be prevented by such simple and cheap means as an external covering of mats or canvass.

The heat of the glass of a hot-house at night cannot exceed the mean of the external and internal air, and taking these at 80° and 40°, 20 degrees of dryness are kept up in the interior, or a degree of saturation not exceeding 528. To this in a clear night we may add at least 6° for the effects of radiation, to which the glass is particularly exposed, which would reduce the saturation to 434°, and this is a degree of drought which must be nearly destructive. It will be allowed that the case which I have selected is by no means extreme, and it is one which is liable to occur even in the summer months. Now by an external covering of mats, &c. the effects of radiation would be at once annihilated, and a thin stratum of air would be kept in contact with the glass which would become warmed, and consequently tend to prevent the dissipation of the heat. But no means would of course be so effective as double glass including a stratum of air. Indeed such a precaution in winter seems almost essential to any great degree of perfection in this branch

of Horticulture. When it is considered that a temperature at night of 20° is no very unfrequent occurrence in this country, the saturation of the air may upon such occasions fall to 120°, and such an evil can only at present be guarded against by diminishing the interior heat in proportion; but whether we run upon Scylla or Charybdis is no very desirable choice.

By materially lowering the temperature we communicate a check which is totally inconsistent with the welfare of tropical vegetation. The chill which is instantaneously communicated to the glass by a fall of rain and snow, and the consequent evaporation from its surface, must also precipitate the internal vapour, and dry the included air to a very considerable amount, and the effect should be closely watched. I do not conceive that the diminution of light which would be occasioned by the double panes would be sufficient to occasion any serious objection to the plan. The difference would not probably amount to as much as that between hot-houses with wooden rafters and lights, and those constructed with curvilinear iron bars, two of which have been erected in the Garden of the Horticultural Society. It might also possibly occasion a greater expansion of the foliage; for it is known that in houses with a northern aspect, the leaves grow to a larger size than in houses which front the south. Nature thus makes an effort to counteract the deficiency of light by increasing the surface upon which it is destined to act.

The present method of ventilating hot-houses is also objectionable, upon the same principles which I have been endeavouring to explain. A communication is at once opened with the external air, while the hot and vaporous atmosphere is allowed to escape at the roof; the consequence is, that the dry external air rushes in with considerable velocity, and becoming heated in its course rapidly abstracts the moisture from the pots and foliage. This is the more dangerous, in as much as it acts with a rapidity proportioned in a very high degree to its motion. I would suggest it as a matter of easy experiment whether great benefit might not arise from warming the air to a certain extent, and making it traverse a wet surface before it is allowed to enter the house.

There is one practice universally adopted by gardeners, which is confirmatory of these theoretical speculations, namely, that of planting tender cuttings of plants in a hot bed, and covering them with a double glass. Experience has shown them that many kinds will not succeed under any other treatment. The end of this is obviously to preserve a saturated atmosphere; and it affords a parallel case to that of Dr. Wells of the anticipation of theory by practice.

The effect of keeping the floor of the hot-house continually wet has been already tried at the Society's Garden, at my

suggestion, and it has been found that the plants have grown with unprecedented vigour: indeed their luxuriance must strike the most superficial observer.

To the human feelings the impression of an atmosphere so saturated with moisture is very different from one heated to the same degree without this precaution; and any one coming out of a house heated in the common way, into one well charged with vapour, cannot fail to be struck with the difference. Those who are used to hot climates have declared that the feel and smell of the latter exactly assimilate to those of the tropical regions.

But there is a danger attending the very success of this experiment which cannot be too carefully guarded against. The trial has been made in the summer months, when the temperature of the external air has not been low, nor the change from day to night very great. In proportion to the luxuriance of the vegetation will be the danger of any sudden check, and it is much to be feared, that unless proper precautions are adopted, the cold, long nights of winter may produce irreparable mischief.

I am aware that a great objection attaches to my plan of the double glass, on account of the expense, but I think that this may appear greater at first sight than it may afterwards be found to be in practice. It is however, at all events, I submit, a point worthy of the Horticultural Society to determine, and if the suggestion should be found to be effective, the lights of many frames which are not commonly in use in winter might, without much trouble, be fitted to slide over the hot-houses during the severe season: and in the spring, when they are wanted for other purposes, their places might be supplied at night by mats or canvass.

The principles which I have been endeavouring to illustrate should be doubtless extended to the pinery and the melon frame, in the latter of which a saturated atmosphere might be maintained by shallow pans of water. An increase in the size of the fruit might be anticipated from this treatment, without that loss of flavour which would attend the communication of water to the roots of the plants.

I have but few additional observations to offer upon the artificial climate of a green-house. The remarks which have been made upon the atmosphere of the hot-house are applicable to it; though not to the same extent. The plants which are subject to this culture seldom require an artificial temperature greater than 45° or 50°, and few of them would receive injury from a temperature so low as 35°. When in the house they are effectually sheltered from the effects of direct radiation, which cannot take place through glass; but the glass itself radiates very freely, and thus communicates a chill to the air, which might

effectually be prevented by rolling mats. With this precaution, fire would be but rarely wanted in a good situation, to communicate warmth. But in this damp climate it may be required to dissipate moisture. The state of the air should be as carefully watched with this view as where a high temperature is necessary, to guard against the contrary extreme. Free transpiration, as I have before remarked, is necessary to the healthy progress of vegetation, and when any mouldiness or damp appears upon the plants, the temperature of the air should be moderately raised, and free ventilation allowed. When the pots in the proper season are moved into the open air, it would contribute greatly to their health to preserve them from the effects of too great evaporation, to imbed them well in moss or litter: as a substitute for this precaution, the plants are generally exposed to a northern or eastern aspect where the influence of the sun but rarely reaches them, but which would be very beneficial if their roots were properly protected. The advantage of such a protection may be seen when the pots are plunged into the soil, a method which communicates the greatest luxuriance to the plants, but unfits them to resume their winter stations.

When a green-house is made use of, as it often is, after the removal of the pots, to force the vine, the same precautions should be attended to as in the management of the hot-house, and the elasticity of the vapour should be maintained by wetting the floor; but after a certain period a great degree of dryness should be allowed to prevail, to enable the tree to ripen its wood, and form the winter protection for its buds. In this its treatment differs from that of the tropical plants, which require no such change, and to which, on the contrary, it would be highly detrimental. The same observation applies to forcing houses for peaches, and other similar kinds of trees. As soon as the fruit is all matured they should be freely exposed to the changes of the weather.

Upon an attentive consideration and review of the subject, it appears to me certain that a frequent consultation of the indications of the hygrometer is quite as necessary to the Horticulturist as of those of the thermometer, and it is not unworthy of the consideration of the Horticultural Society, whether correct registers of the state of the climate, both in their houses and out of doors, and a connected series of experiments upon the modifications of which it is susceptible, might not contribute something to the perfection of that art, which they are making such honourable exertions to perfect and communicate.

To me it will be a source of great satisfaction if any observations which I have made, or may make, upon the subject of climate, should prove to be at all instrumental in forwarding their important views.

Article X.

*On the Means of detecting Lithia in Minerals by the Blowpipe.** By Edward Turner, MD. FRSE. &c. Lecturer on Chemistry, and Fellow of the Royal College of Physicians, Edinburgh. (Read before the Royal Society of Edinburgh, Dec. 5, 1825.)

At the conclusion of a paper on mica, published in the last number of the "Edinburgh Journal of Science," I have made some observations on the colour communicated to the flame of a candle by the three alkalies, potash, soda, and lithia, by means of which they might be readily distinguished from each other. It seemed probable, from some facts there stated, that a body must be fluid, in order to communicate its characteristic colour to flame; and this idea became more plausible from the consideration, that the lithion-micas fuse readily, and then tinge the flame red, while some other minerals which do not produce that effect, though they contain lithia in considerable quantity, are very difficult of fusion. Hence it occurred to me, that the last description of minerals might also be made to redden flame, could we by any means increase their fusibility; and the following observation is in support of this notion.

A minute particle of spodumene, previously reduced to fine powder, and made into a paste with water, was exposed to the flame of the blowpipe. For a time the mineral did not fuse, nor was a trace of redness visible; but by urging the heat, fusion did at length occur, and at that instant the flame was tinged of a red colour, though in a slight degree. On mixing the same mineral with fluor spar, its fusibility was considerably increased, and it gave a more distinct red hue to the flame.

But though the liquid form is favourable to the communication of colour to flame, it is not always an essential condition. Thus the carbonate of copper tinges the flame of a candle green without fusing; and if the carbonate of strontia be strongly heated before the blowpipe, it phosphoresces remarkably, and yields a red colour to the flame, though the assay remains perfectly solid. Nor does a body cause its peculiar colour to appear from the mere circumstance of becoming fluid. Spodumene, for example, can be made to fuse by the addition of the carbonate of soda or potash, but no redness occurs. Fusion is rendered still more perfect by the action of boracic acid, or the phosphate of soda and ammonia, but without a trace of redness being visible.

These facts prove that a certain chemical condition of a body is necessary, in order that it should produce its effect on flame, and that this circumstance has a greater influence than form.

* Edinburgh Journal of Science.

From the action of fluor spar on spodumene, I was desirous of trying the effect of free fluoric acid on that mineral. It was accordingly mixed with some of the bifluate of potash, and a little of the mixture, made into a paste with a drop of water, was exposed by means of platinum wire to the flame of the blowpipe. It fused very easily, and emitted a brilliant red flame, far more distinct than that occasioned by the fluate of lime. To vary the experiment still further, a mixture was made, composed of fluate of lime, and bisulphate of potash in atomic proportion; that is, one of the former to about four and a half of the latter. When this flux was mixed with an equal quantity of spodumene, the effect was, if any thing, still greater than in the previous instance. Both these fluxes appear to act by giving out fluoric acid at a high temperature, which destroys the composition of the mineral by combining with the silica, and setting the lithia free. The latter flux is more effectual than the former, because it requires a stronger heat before yielding fluoric acid, and hence the disengagement takes place under the most favourable circumstances. It should therefore be preferred in practice.

In performing these experiments, it is important to keep in view the action of the flux itself on flame. Those that have been just recommended communicate a faint lilac colour, owing to the presence of potash, which cannot be mistaken for the action of lithia by any one who compares both effects together, as I shall immediately demonstrate to the Society. But in case any doubt should arise, it is easy to avoid the difficulty by employing a flux that contains no potash. Such a one may be made by mixing one part of the fluate of lime with one and a half of the sulphate of ammonia. This mixture acts on spodumene in the same way as the preceding, and doubtless from the same cause. It communicates a pale bluish-green colour to the flame at the first moment, and before fusion occurs—a property possessed by several of the salts of ammonia; but there is no appearance that can be mistaken for the red colour of lithia.

When petalite is heated alone before the blowpipe, it yields no trace of redness; but if subjected to the process just recommended, it affords abundant evidence of the presence of lithia. Indeed, from the great affinity of fluoric acid for silica, it is obvious that no siliceous mineral can withstand its action; and there can be almost as little doubt that the presence of lithia may be detected in any such compound by the process which is so successful with spodumene and petalite.

The advantage of possessing an easy and expeditious method of ascertaining the presence of lithia in mineral bodies is twofold. In the first place, the mineralogist and chemist possesses a test for spodumene and petalite, from the want of which other minerals have sometimes been mistaken for them, and the error

only discovered at the close of a tedious chemical process. Secondly, we obtain a method of ascertaining the presence or absence of lithia in other minerals. I have examined a considerable number of substances with this view, but have not hitherto been successful.

As several of the salts of strontia and lime possess the property of communicating a red colour to flame, it is natural to inquire, whether the presence of those earths in a mineral might not give rise to fallacy; and I have accordingly studied the subject with care. Though there is little danger of mistaking a native carbonate or sulphate of strontia for a siliceous mineral containing lithia, it may not be superfluous to mention the characters they exhibit before the blowpipe. When a particle of strontianite, powdered and made into a paste as usual, is exposed on platinum wire to the blowpipe flame, it communicates a yellowish colour to it. By continuing the blast for a little time, phosphorescence commences, and soon afterwards a red colour makes its appearance. This latter effect depends on the expulsion of carbonic acid; for no redness is visible till the phosphorescence sets in, and then the assay gives a strong brown stain to moistened turmeric paper. The property of strontianite in colouring flame is lessened by mixing it with the flux. When celestine is exposed in like manner, no redness appears at first; but if a strong heat be kept up for a minute or two, the salt is decomposed, phosphorescence commences, followed by a red hue, and the assay is found to be alkaline. This change is facilitated by mixing the celestine with the flux of bisulphate of potash and fluor spar. Complete fusion then occurs, though without the least trace of a red colour; but on continuing the blast, the assay gradually becomes solid, and then the strontia is speedily reduced to the caustic state. I have been thus particular in describing these appearances, because they afford us a useful test to distinguish the native salts of strontia from those of baryta; while they cannot be confounded with the effects produced by lithia.

The carbonate and sulphate of lime give rise to the same phenomena, though the effect is less distinct; and the colour, as in the case of strontia, does not appear till the lime is reduced to its caustic condition. I have examined a considerable number of siliceous minerals containing lime, in some of which, as datolite and apophyllite, that earth is present in a large proportion; but none of them, whether alone or with flux, give a red colour to the flame of the blowpipe. It is probable, from this fact, that strontia, did it chance to occur in a siliceous mineral, would likewise be inert; or if it did redden the flame, it would be under circumstances which would distinguish it from the action of lithia. For the strontia would be converted into a

sulphate by the flux, and would not produce its effect till that salt was decomposed.

It is very desirable that the presence of potash and soda in minerals could also be discovered by the blowpipe. The pale lilac produced by potash, though it enables a salt of that alkali to be readily distinguished from the salts of soda or lithia, is too faint for affording a test of its presence in minerals, unless it exists in considerable quantity. The property soda possesses of communicating a yellowish colour, and of making the flame larger at the same time, may be turned to some advantage; for several minerals that contain soda act on the blowpipe's flame in the same manner as soda itself, from which we may be led to infer the presence of that alkali in them. This has been observed in sodalite, analcime, chabasie, albite, pitchstone, and several others. Unfortunately, however, a yellowish colour may be produced by other substances besides soda, so that it is a test which cannot altogether be relied on with certainty. Thus, a similar effect is occasioned, though in a less degree, by the fluate of lime, and, perhaps, by lime under other circumstances. However this may be, it is certain that many minerals that contain soda give a very distinct yellow colour to flame; it is a circumstance, therefore, which may be useful to the chemist and mineralogist, and as such I mention it.

I beg leave to observe, in conclusion, that experiments on the colour communicated to flame should be performed with a tallow candle, the colour of which is better fitted for the purpose than that of a spirit-lamp.

Article XI.

Description of a new Species of Grosbeak, inhabiting the North-western Territory of the United States. By William Cooper.*

The genus *Loxia* being restricted by the most eminent modern ornithologists to the *Crossbills*, the remaining species of graniverous birds having a conical, straight, and pointed bill, and which were arranged by Linnæus, and the authors who have followed him, as a species of *Loxia*, are now all comprehended under the genus *Fringilla*. The number of species thus brought together is consequently very great; but they present such a gradual passage from one character to another, that it is found impracticable to separate them into well defined and natural genera. In order to avoid, however, the inconvenience which would result from so many species being compre-

* From the Annals of the Lyceum of the Natural History of New York.

hended under one head, Temminck proposes to subdivide them into three sections, characterized by the forms of their bills, viz. *laticones, brevicones,* and *longicones.* This simple arrangement appears preferable to the multitude of artificial genera which some nomenclaters have attempted to establish; or it would perhaps be more convenient to consider these sections as subgenera, under the names of *Coccothraustes, Fringilla,* and *Carduelis,* as suggested by Mr. Charles Bonaparte, in his judicious *Observations on the Nomenclature of Wilson's Ornithology.** The first of these comprises nearly all the *Loxiæ* of authors which are not *Crossbills,* and to it must be referred the species which is the subject of this notice.

FRINGILLA VESPERTINA, (nobis.)

F. fronte flavo; vertice alis caudâque nigris; macula alarum alba.

DESCRIPTION. *Length* 8½ inches: *bill* yellow; strong, conical, swelling, pointed, depressed above at the base where it forms an angle in the feathers of the front; the upper mandible turning a little downwards at the point, and slightly notched, edges of both sharp and cutting: *nares* roundish, partly concealed by the black vibrissæ: a narrow black line surrounds the base of the upper mandible: *front* and a band passing over and behind the eyes yellow; *crown of the head* black: *cheeks* brown: *throat* and *neck* olive-brown passing downwards into yellow, which is the colour of the *back* and all the *under parts of the body, flanks* and *tail coverts*: *lesser wing coverts, primaries* and *first secondaries* black: *greater wing coverts* and *inner secondaries* white on their lower half, forming a large white spot on the wing; the first three *primaries* of equal length and longer than the rest: *tail* of 12 feathers, slightly forked, entirely black: *feet* light brown.

Cabinet of the Lyceum.

OBSERVATION. In the natural series of American species, this bird should be arranged as the first of the series; being intermediate between the *F. coccothraustes* of Europe and the *F. cardinalis.* To the former of these, which is the type of the subgenus, it is nearly allied, but is considerably larger, besides other differences, as the description given will show.

The specimen from which this description is taken, was sent to the Lyceum from Sault Ste. Marie, near Lake Superior, by Mr. H. R. Schoolcraft, and is labelled *Paushkundamo,* the name given it by the Chippewa Indians. Mr. Schoolcraft has since favoured me with the following account. It is a little singular that the meaning of the Chippewa name should so

* Journal of the Academy of Natural Sciences, vol. iv. p. 39.

nearly coincide with that of the subgenus in the language of the system.

"*Paushkundamo.*" This word is derived from the Chippewa verb *paushka-un,* to break. The termination indicates the object acted on, and is in accordance with one of the rules of their language, which permits the formation of compound words from a verb and substantive, dropping one or more syllables of each for the sake of euphony. The word *paushkaun* is the *animate* form of the verb, and is used only in particular reference to soft, fleshy, or vegetable substances, as a fly, a berry, &c. The word will therefore admit of being rendered *fly-breaker, berry-breaker,* &c.

"This bird appeared about Sault Ste. Marie, M. T. during the first week in April 1823. The individual under examination was shot on the 7th April, in the *evening.* An Indian boy was attracted into the woods by its peculiar, and to him strange note. There were a few birds in company: they were seen for a short time about the place; but none have since appeared. The species is said to be common about the head of Lake Superior, at Fond du Lac, &c."

Major Delafield, in the execution of his duties as agent of the United States for boundaries, met with the same bird in the month of August 1823, near the Savannah river, north-west from Lake Superior, and has obligingly communicated the following extract from his notes made at the time.

"At twilight, the bird which I had before heard to cry in a singular strain, and only at this hour, made its appearance close by my tent, and a flock of about half a dozen perched on the bushes in my encampment. They approached so near, and were so fearless, that my canoe-men attempted to catch them, but in vain. I recognised this bird as similar to one in possession of Mr. Schoolcraft, at the Sault Ste. Marie.

"Its mournful cry about the hour of my encamping, (which was at sunset) had before attracted my attention, but I could never get sight of the bird but on this occasion. There is an extensive plain and swamp through which flows the Savannah river, covered with a thick growth of sapin trees. My inference was then, and is now, that this bird dwells in such dark retreats, and leaves them at the approach of night."

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Dec. 22 (*continued*).—A paper was read, On the Poison of the Common Toad; by J. Davy, MD. FRS.

The popular belief in the venomous nature of the Toad, Dr. Davy states, though of great antiquity, has been rejected as a vulgar prejudice by modern naturalists, decidedly so by Cuvier; but like many other long received and prevalent opinions, it is a true one, and the denial of it by philosophers has resulted from superficial examination. Dr. D. found the venomous matter to be contained in follicles, chiefly in the cutis vera, and about the head and shoulders, but also distributed generally over the body, and even on the extremities. On the application of pressure, this fluid exudes, or even spirts out to a considerable distance, and may be collected in sufficient quantity for examination. It is extremely acrid when applied to the tongue, resembling the extract of aconite in this respect, and it even acts upon the hands. It is soluble, with a small residuum, in water and in alcohol, and the solutions are not affected by those of acetate of lead and corrosive sublimate. On solution in ammonia, it continues acrid; it dissolves in nitric acid, to which it imparts a purple colour. By combination with potash or soda, it is rendered less acrid, apparently by partial decomposition. As left by evaporation of its aqueous or alcoholic solutions, it is highly inflammable; and the residuary matter that appears to give it consistence seems to be albumen. Though more acrid than the poison of the most venomous serpents, it produces no ill effect on being introduced into the circulation; a chicken inoculated with it was not affected.

The author conjectures that this "sweltered venom," as it is correctly termed by our great Dramatist, being distributed over the integuments, serves to defend the Toad from the attacks of carnivorous animals: "to eat a toad" has long been held as an opprobrious difficulty; and the animal is still further protected in this respect by the horny nature of its cutis, which contains much phosphate of lime, &c. As the venom consists in part of an inflammable substance, it is probably excrementitious, and an auxiliary to the action of the lungs in decarbonizing the blood. This view of its use is confirmed by the fact that one of the two branches of the pulmonary artery supplies the skin, its ramifications being most numerous, where the follicles of venom are thickest.

Dr. Davy has found the skin of the Toad to contain *pores* of two kinds; the larger, chiefly confined to particular situations, and which, when the skin is held up to the light, appear as iridescent circles, and the smaller, more numerously and generally distributed, which appear as luminous points of a yellowish colour. Externally these pores are covered with cuticle, and some of the larger ones even with rete mucosum; internally they are lined with delicate cellular tissue. By inflating the skin, Dr. D. ascertained that it was not furnished with spiracula, the existence of which he had been led to suspect by some particular circumstances in the physiology of the animal.

A paper, by the same author, was also read, On the Heart of Animals belonging to the Genus Rana. Dr. Davy has discovered that the heart of the Common Toad, the Bull Frog, and the Common Frog, instead of consisting of one auricle and one ventricle, as generally stated, has two auricles, divided by a septum of fibrous substance; and he has reason to believe that this structure prevails throughout the order of *Batraciens*. This discovery removes the anomaly among Reptiles supposed to be presented by these animals, as forming a portion of the link between Mammifera and Fishes, and preserves unbroken the chain of connection between Reptiles and Fishes arising from the analogy of their respective organs of respiration.

Jan. 12, 1826.—Dr. T. S. Tiarks, and Sir C. Wetherell, Knt. his Majesty's Solicitor-General, were respectively admitted Fellows of the Society.

A paper was read, entitled, "Observations on the Heat of July, 1825, together with some Remarks on sensible Cold; by W. Heberden, MD. FRS."

These observations were made on the author's lawn at Datchet, in Berkshire, by means of thermometers suspended in the shade of trees: the highest temperature observed was 97° Fahr. Dr. Heberden remarks, that the extraordinary weather of this month passed away without rain, lightning, change of wind, or any other obvious cause; and cites the nearly parallel case of the year 1808, as recorded in the Society's observations, and also by Mr. Cavendish. He gives some observations and experiments on a method of ascertaining the *sensible heat*, which he believes to be much above that indicated by the thermometer, by means of previously raising the thermometer to a high temperature, and then noting its successive decrements in equal times on exposing it to the open air. In the concluding remarks on sensible cold, Dr. H. states his opinion that its chief cause is the loss of heat by the body effected by the action of the wind, not by the moisture of the surrounding atmosphere.

A communication was also read, entitled, "An Account of a Series of Observations to determine the Difference of Longitude between the National Observatories of Greenwich and Paris; by

J. F. W. Herschel, Esq. Sec. RS.: communicated by the Board of Longitude."

In this paper, after stating the wish expressed by the French Ministry of War, that the above determination should be made, with the ready accession to their desire of our own Board of Longitude, and describing the method resorted to, Mr. Herschel gives the observations in detail. They were made by himself and one French officer on this side of the Channel, and by Capt. Sabine and another French officer on the coast of France. Their general result is $9' \, 21\frac{6}{10}''$ for the difference of longitude between the two Observatories; and though many of the observations had been rendered unavailable by untoward circumstances which it was impossible to foresee or to obviate, Mr. H. stated that this determination was not likely to require a correction exceeding 1-10th of a second, and very unlikely to want one of twice that amount.

Jan. 19.—The Right Hon. George Canning, his Majesty's Principal Secretary of State for Foreign Affairs, having been elected into the Society at the preceding meeting, but being unable to attend for admission, on account of the pressure of public business, requested that his name might be inserted in the printed lists of the Society, which was granted accordingly. S. H. Christie, Esq. was admitted a Fellow of the Society; and the following papers were read:

On the Cambridge Transit Instrument, being a supplement to a former paper; by Prof. Woodhouse, FRS.

On the Magnetic Influence of the Solar Rays; by S. H. Christie, Esq. AM. FRS.

Jan. 26.—N. B. Edmonston, Esq. was admitted a Fellow of the Society, and a paper was read, On the Barometer; by J. F. Daniell, Esq. FRS.

GEOLOGICAL SOCIETY.

Nov. 4.—A paper was read entitled, An Account of some Geological Specimens collected by Capt. P. P. King, in his Survey of the Coasts of Australia; and by Robert Brown, Esq., on the Shores of the Gulf of Carpentaria, during the Voyage of Captain Flinders. By W. H. Fitton, M.D. V.P.G.S. &c.

The survey of Captain King commenced on the north east coast of Australia, about the latitude of 21° south, and proceeded northward and westward (omitting the Gulf of Carpentaria previously examined by Capt. Flinders), and southward, on the western shore, to about the latitude of 25°, where the coast had been examined by the French expedition under Capt. Baudin. The chasm in Capt. King's specimens has been supplied by those collected on the shores of the Gulf of

Carpentaria, by Mr. Brown, who accompanied Capt. Flinders in his survey of the coasts of New Holland.

The land visible from the sea on the north-east coast, is in general mountainous, as far north as Cape Weymouth, between the latitudes 12° and 13° (South):—A high and rocky range especially, which begins about latitude 25°, being continued northward in a direction nearly parallel to the shore, for more than 150 miles without interruption. The outline and aspect of this range, and of several other groups of mountains, are irregular, and resemble those of primitive tracts; peaked summits also are of frequent occurrence both on the main land and the adjacent islands. Mount Dryander, about latitude 20° 12′, one of the chief mountains, is nearly 4500 feet high; Mount Hinchinbroke, lat. 8° 22′, more than 2000 feet; and several other mountains in this quarter are of considerable elevation.

Along this part of the coast, granite has been found, in detached points, through a space of about 500 miles: and rocks of the floetz-trap formation occur in several of the islands off the shore.

The coast-line, on the north of latitude 14°, is thrown back about 40 miles to the westward of its previous course; and, about the same point, the elevation of the land declines: the general height of the main land about Cape York, the north-eastern point of Australia, is not more than 400 or 500 feet.

The eastern shore of the Gulf of Carpentaria, occupying a space of about 500 miles from north to south, is very low, and very uniform in its outline. The rock on the shore at Coen's River, the only point examined upon this coast, was found to be calcareous sandstone of recent formation. The western shore of the gulf is more broken, and of higher level; and the specimens from thence consist of granite and primitive slaty rocks; upon which repose quartzose sandstone and conglomerate, identical in character with the rocks which are found in great abundance further to the west on the northern shore, and on the north-west coast, and with the most ancient sandstones and conglomerates of Europe. Clink-stone also, and other rocks of the trap-formation, occur among the specimens from the islands in this part of Australia: but the chains of islands which form the north-western verge of the Gulf of Carpentaria, and are remarkable for the similarity of their structure, and their uniform direction, appear to consist principally of quartzose sandstone and conglomorate, reposing upon primitive rocks. The main land of the north coast, from about longitude 135°, to Melville Island about 131°, is in general low, and is interrupted by two considerable streams named Liverpool and Alligator's rivers; the last of which consists, in fact, of three separate branches. The specimens from Goulburn's Islands on

the north of this part of the coast are composed of reddish quartzose sandstone.

One of the most remarkable inlets on the north-west of Australia is Cambridge Gulf, about longitude 121° and latitude 15°: it has been traced to more than 60 miles from the sea, between hills from 150 to 400 feet in height, which have in general flat summits, and are composed of sandstone of a reddish hue, and of the same characters with that already mentioned. The specimens from Lacrosse Island, at the entrance of this gulf, are not to be distinguished from the slaty strata of the old red-sandstone, which occur in the banks of the Avon, between Clifton and the Severn.

The outline of the north-west coast is remarkably broken, and the adjacent sea is studded with very numerous islands; the forms of which, as well as of the hills on the main land, are remarkable for their flat summits. In two detached points about 70 miles apart, Port Warrender and Careening Bay, epidote has been found in considerable quantity, both crystallized in veins, and in a compact form as a component in a rock of a conglomerated and amygdaloidal structure. Prince Regent's river, the chief inlet of the north-western shore, has nearly a rectilinear course, from north-west to south-east, for more than 60 miles; and its banks of sandstone are in some places between 300 and 400 feet high. The coast to the south-west of this inlet has not yet been completely surveyed; but several openings have been observed there, of such width as to render the existence of rivers not improbable.

The shore on the western coast is in several places covered with extensive dunes of sand, with which are associated in many instances beds and masses of a very recent arenaceous breccia, abounding in shells concreted by carbonate of lime. This formation, which is particularly remarkable in the islands and on the shores adjacent to Shark's Bay, about latitude 25°, is analogous to that which occurs very extensively in Sicily, at Nice, and several other places on the shores of the Mediterranean, and of the West India Islands, and on many parts of the coasts within the Tropics. In New Holland it generally consists of sand cemented by stalagmitic or tufaceous carbonate of lime, containing angular fragments of a compound of the same nature, but previously consolidated and broken, along with numerous shells and fragments of shells, very nearly resembling those of the adjacent seas. Its date appears to be more recent than that of the beds which constitute the Paris and London basins; but anterior to the accumulation of the diluvial gravel.

The calcareous concretions of New Holland have in some instances a tubular and stem-like appearance; and have thence been mistaken for corals, and petrified branches of trees.

On a general view of the north and north-west of New Holland, it will be observed that the outline of the coast, in several distant quarters, has a direction nearly uniform, from south-west to north-east; which is the course also of the remarkable ranges of islands on the north-west of the Gulf of Carpentaria. It appears also that reddish sandstone of ancient date is very abundant throughout the north and west coasts; and it is not altogether improbable that the prevailing direction of the strata may be that above-mentioned.

So little is known of the remainder of Australia, and especially of the interior, that speculations upon its general structure would bepremature; but the linearity of the coast lines in several other places is remarkable; and their course, as well as that of the principal openings, has also a general tendency to a direction from the west of south toward the east of north. The coincidence of uniformity of range with marked features of geological constitution, is of such frequent occurrence in other parts of the globe, that these appearances are in the present case deserving of attention; but they are mentioned by the author, under the existing scantiness of information from Australia, merely as suggesting ground for more extensive inquiry.

Nov. 18.—A notice was read, respecting the appearance of Fossil Timber on the Norfolk coast; by Richard Taylor, Esq., of Norwich.

In consequence of an extraordinary high tide which visited the coast of Norfolk on the 5th of February last, large portions of the cliffs, sometimes exceeding 200 feet in height, were precipitated into the sea, and an opportunity was afforded of examining the site of a stratum containing a number of fossil trees, exposed on the east and west side of the town of Cromer. In this singular stratum, composed of laminæ of clay, sand, and vegetable matter, and about four feet in thickness, the trunks were found standing as thickly as is usual in woods, the stumps being firmly rooted in what appears the soil in which they grew. They are invariably broken off about a foot and a half from the base. The stem and branches lie scattered horizontally; and amongst them are thin layers of decomposed leaves, but no fruits or seed-vessels. The species of timber appear to be chiefly of the Pine tribe, with occasional specimens of elm and oak: they are flattened by the pressure of the overlying alluvial strata. Mr. Taylor has not observed any animal remains in the stratum, except a skull of one of the Deer tribe; but he supposes that the bones of Elephants and other herbivorous animals, found near this site, may have been washed out of the same bed.

An extract of a letter from the Right Hon. Earl Compton, FGS. to the President, was read, On the Discovery of Granite with Green Felspar found in Excavations at Tivoli. In excava-

tions made during the spring of 1825 at Tivoli, on the spot where the villa of Manlius Vopiscus stood, fragments of granite were discovered, the felspar of which is of a green colour, exactly resembling that which is called Amazonian stone. As this rock was never before known to be among those employed by the ancients, it becomes a curious point, observes the author, to ascertain whence they derived it, since the modern localities of the Amazonian stone are confined to Siberia and the continent of America. As Egyptian hieroglyphics appear on the original surface of some of these fragments, Lord Compton supposes the green granite to have been found, though as a very rare substance, in Egypt.

A paper was also read, entitled, "Notice of Traces of a Submarine Forest at Charmouth, Dorset; by H. T. de la Beche, Esq. FRS. GS. &c."

A circumstance seeming to indicate the existence of the remains of a submarine forest near the mouth of the Char, was lately pointed out to Mr. De la Beche by Miss Mary Anning. Upon a flat of some extent, stretching into the sea in front of the beach, only visible at low water, and composed of lias, patches of a blue clay show themselves, imbedding pieces of blackened wood lying horizontally, similar in appearance to those usually met with in submarine forests; some of them are large, but the greater number must have been derived from small trees; mixed with these are a few hazel nuts, and abundant remains of plants, chiefly such as are found in marshy grounds. Angular and blackened pieces of chert and flint, precisely resembling those which occur in the diluvium on either side of the Char, form the substratum of this clay, which has been worn away on most places by the rolling of the large pebbles thrown up by the action of the sea upon the beach.

Dec. 2.—A paper, entitled, "Remarks on the Geology of Jamaica; by H. T. De la Beche, Esq. FGS." was read in part.

A paper was also read, entitled, "An Account of an undescribed Fossil Animal from the Yorkshire Coal-field; by John Atkinson, FLS. and Edward Sanderson George, FLS."

Dec. 16.—A paper was read, "On the Chalk and Sands beneath it (usually termed Green Sand), in the Vicinity of Lyme Regis; by H. T. De la Beche, Esq. FGS. &c."

Mr. De la Beche observes that we ought not to suppose that the sands, marles, and clays, which are immediately subjacent to the chalk in the east of England, can be traced into other and distant countries, where however these sands, &c. as a mass, may be easily recognized. That this cannot be done, even at comparatively short distances, it is the object of this communication to prove, by examples derived from the cliffs at Lyme Regis, in Dorsetshire, and Beer, in Devonshire; detailed sections of which

are given, and the succession of the strata, and the organic remains which they contain fully described. The author first treats of the chalk, and the sands and sandstone, usually called green sand, as they occur between Lyme Regis and Axmouth; and then notices the same formations as they are exhibited in the vicinity of Beer.

From this examination it appears, that though there is a great correspondence in the organic remains, considerable changes take place in the mineral composition and characters of the beds both of chalk and underlying sands, in short distances. Mr. De la Beche considers it probable that the Beer-stone is the equivalent of the Malm-rock of Western Sussex.

A paper was also read, entitled, "Geological Sketch of Part of the West of Sussex, and the NE of Hants, &c.; by R. I. Murchison, Esq. FGS. &c."

In this memoir, Mr. Murchison describes the geological relations, distribution, and characteristic fossils of the strata of that part of the west of Sussex, which is bounded on the south by the chalk escarpment of the South Downs; and of that part of Hampshire which is included by the Alton Chalk Hills. These strata, commencing below the chalk, in a descending series, are, 1. Malm-rock, or Upper Green Sand; 2. Gault; 3. Ferruginous Green Sand; 4. Weald Clay. The Weald clay in the valley of Harting Combe, may be regarded as the central nucleus of this district, mantling round which, and extending up to either chalk range, the other formations are developed, in regular succession: the breadth and boundaries of each are laid down by the author on a coloured portion of the Ordnance map, to which a section is annexed.

The malm-rock of Western Sussex is identical with the stone of Merstham: it is characterized by constituting terraces which afford a rich soil favourable to wheat. It sometimes furnishes a building stone, contains occasionally a calcareous blue chert, and abounds in organic remains.

The gault of this district has been cut through to the depth of 120 feet, at Alice Holt, and iridescent Ammonites and other fossils are found in it. This clay is marked by fertile water-meadows, and the timber presenting a green belt clearly distinguishes it from the rich wheat land of the malm rock above, and the arid expanse of the ferruginous green sand below it.

Of this latter formation, the upper beds consist of pure white sand, and in some places compact ironstone, and ironstone in large cellular tubes are found. In the middle beds occurs a calcareo-siliceous grit, called Bargate stone; in the lower, a siliceous yellow building stone, containing casts of Ammonites, Terebratulæ, &c. The Weald clay includes in its middle beds the compact Petworth marble, and in lower beds of clay in which

tabular calcareous grit occurs, Mr. Murchison has discovered, together with scattered shells of the *Vivipara fluviorum*, the bones of a large unknown vertebrated animal, specimens and drawings of which accompany this memoir.

Jan. 6, 1826.—The reading of Mr. De la Beche's paper on the Geology of Jamaica, was continued. E. W. B.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Prof. Berzelius's Discovery of Lithia in Mineral Waters.*

Prof. Berzelius has been occupied with the examination of several mineral waters from Bohemia, viz. those of the Eger, or Frauzensbad, and those of Marienbad. These waters were found to contain the same substances which this chemist detected in those of Carlsbad, the analysis of which has been for some time before the public,* but in the new analysis he has found no lithia. The quantity of the carbonate of the alkali is very small, particularly in the waters of Carlsbad, and in that of Eger; but the waters of the spring called Kreuzbrunn, at Marienbad, contain as much as a centigramme of the carbonate of lithia in every bottle.

The following is M. Berzelius's method of discovering this alkali in any solution. He precipitates the lime by means of oxalate of potash, and afterwards separates the magnesia by carbonate of soda; but the mixture must be evaporated to dryness, and the residue fused; for otherwise some of the magnesia would be easily redissolved in the form of a double carbonate of soda and magnesia. The mass taken up by the water and filtered will not give any further precipitation even when pure phosphate of soda is added; but if it contain lithia it will become turbid during the evaporation, which must be continued till the matter be perfectly dry. It is next redissolved in a very small quantity of cold water, which leaves undissolved a double phosphate of soda and lithia, equivalent to one-third of its weight of carbonate of lithia. The characters which distinguish this phosphate from the earthy phosphates with which it may be confounded, are as follows: It is very fusible before the blowpipe. When melted with carbonate of soda, it enters with the soda into the charcoal. On a leaf of platina the melted mixture is limpid. The earthy phosphates remain on the charcoal while

* See *Annals of Philosophy*, vol. viii. N. S. p. 123.

the soda penetrates it, and do not give a limpid mixture when they are melted on a leaf of platina. With twice its weight of carbonate of lime, it fuses at a red heat, without however attacking the platina, as lithia ordinarily does; but if some drops of water are added to it, and afterwards evaporated, the platina becomes yellow all round when the mass is heated anew. —(Edin. Jour. of Science.)

2. *Prof. Berzelius's Experiments on the Orange Gas produced from a Mixture of Fluor Spar and Chromate of Lead.*

As the English and French journals have already given an account of Prof. Berzelius's experiments on the different combinations of the fluoric acid which have facilitated the reduction of silicium, zirconium, and tantalum, we shall not at present enter on the subject.

A German chemist, M. Unverdorben, has published some experiments on the fluoric acid, the most curious of which was that in which, after mixing together fluor spar and chromate of lead, he distilled them in a leaden retort, with fuming or anhydrous sulphuric acid. From this there resulted a gas, which could not be collected, because it destroyed the glass. This gas gave a very thick yellow or red smoke. It was readily absorbed in water, which was then found to contain a mixture of chromic and fluoric acids. When it came in contact with air, the gas deposited small red crystals, which were those of chromic acid.

Prof. Berzelius repeated these experiments of M. Unverdorben, and he found that the experiment succeeded equally well with common concentrated sulphuric acid. He collected the gas in glass flasks covered with melted resin, and filled with mercury. The gas had a red colour. It gradually attacks the resin, deposits chromic acid in its mass, and penetrates even to the glass, which it decomposes without change of volume, the chrome being replaced by silicium. Ammoniacal gas introduced into it burns with explosion. Water dissolves it, and yields an orange-coloured fluid, which, evaporated to dryness in a platina dish, leaves as a residue pure chromic acid. The fluoric acid volatilizes entirely. This method is at present the only one which gives chromic acid perfectly pure.

If the gas is received in a platina vessel of some depth, whose sides have been slightly wetted, and into the bottom of which the gas has been made to descend, the water begins to absorb the gas, but, by and bye, crystals of a fine red colour are seen to form themselves round the opening of the metallic tube which conveys the gas, and, in a short time, the vessel is filled with a red snow, consisting of crystals of chromic acid. The fluoric acid dissipates itself in vapour, and absorbs entirely the water added at the beginning of the experiment. These crystals have this curious property, that, when they are heated to redness in a

platina dish, they begin at first to melt, and afterwards, by a slight explosion, accompanied by a flash of light, they decompose themselves into oxygen gas, and the green protoxide of chrome. The chromic acid which has been dissolved in the water does not present this phenomenon. It fuses during its decomposition, but it does not give a flash of light. This difference does not arise from its containing water, for it is perfectly free from it when it is heated to a little above 100° centigrade.

M. Unverdorben had already observed, that crystals of chromic acid introduced into ammoniacal gas, are decomposed with a flash of light. The ammonia is destroyed, and the acid leaves the protoxide as a residue. It is necessary to make these experiments quickly, as the crystallized acid is deliquescent.

In distilling chromate of lead with chloride of sodium, we obtain a gas similar to the preceding, and which contains chrome combined with chlorine in such proportions that the water, by its decomposition, gives rise to the formation of the hydrochloric and chromic acids. The gas is red, and may be collected over mercury, but it is very much charged with chlorine, when it is prepared by means of the common concentrated sulphuric acid, whose water of combination destroys a certain quantity of gas. —(Edin. Jour. of Science.)

Mineralogy.

3, *Arseniate of Iron.*

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Glasgow, Jan. 9, 1826.*

In the January number of the *Annals*, p. 23, there is inserted a letter of Prof. Berzelius to M. A. Brogniart, in which that excellent practical chemist gives the result of his analysis of two varieties of arseniate of iron, and you express, in a note, some doubt about the meaning to be affixed to Berzelius's statement. The French expression which you transcribe is obviously nonsense; owing probably to a typographical error in the periodical work from which you copied it. Luckily you have given us Berzelius's formulæ, from which the constituents of the minerals, according to his view of the subject, are sufficiently obvious. Perhaps I may make the nature of these arseniates obvious to the readers of your work by translating his formulæ into English.

1. The Villa Ricca specimen is a compound salt composed of

1 atom protarseniate of iron,
2 atoms perarseniate of iron,
12 atoms water.

2. The second species is the cube ore of mineralogists; for I need hardly state that the word wurfelerz is not the name of a

place, but a German word meaning cube ore. The mineral occurs in Cornwall, had been analyzed before by Chenevix and Vauquelin, and has been long known to British mineralogists. It is also, according to Berzelius, a compound salt, and its constituents are,

1 atom subsesquarseniate of protoxide of iron,
2 atoms subsesquarseniate of peroxide of iron,
18 atoms water.

I do not give the numbers corresponding to these atoms, because the opinion which Berzelius entertains respecting the composition of arsenic acid, and which doubtless influenced his analysis, is not the same as mine.

I may mention that the mineral called *mesola* in the notice which you have inserted in the *Annals*, is called *Mesolin* by Berzelius, in his new mineralogical arrangement printed in the Memoirs of the Swedish Academy for 1824.

I am, &c. Thomas Thomson.

4. *Fall of Aerolites.*

On the 15th of January, 1824, between nine and ten o'clock in the evening, a fall of aerolites happened in the lower part of the Commune of Renalzo, distant 21 miles from the town of Cento, in the province of Ferrara. This phænomenon was preceded by the appearance of a bright light, which terminated in flashes of lightning. Next were heard, for some miles around, three loud explosions, like discharges of artillery, which were immediately followed by a noise similar to the firing of musquetry, and was distinctly audible as far as the town of Cento. In a short time this noise changed to a sound like that of a number of bells ringing all at once. At last some stones fell with violence, accompanied by a whistling noise, and notwithstanding the darkness of the night, the direction of their fall could be determined, which led to the discovery of the stones themselves. It is said that three of these aerolites have been found. The phænomenon lasted about 20 minutes. The stones were found at an interval of about a mile asunder. Some persons speak of a black cloud which at first appeared in the south-east, from whence it took an oblique direction towards a black body, apparently of the size of a common cauldron, and at length becoming luminous, presented to the eye and ear the phænomena above described. Prof. Ranzani possesses one of the aerolites, which is said to weigh about a pound and a half. They are blackish; externally their colour is lighter, and internally they are sprinkled with brilliant points of the colour of iron, with globules of the same colour still more brilliant, and with small, whitish, round corpuscles, with indeterminate facets, and varying in diameter from one-sixteenth of a line to a line. Further particulars are

expected from the Abbe Ranzani, and also the analysis of the stones by Dr. Santagata.—(Bullet. des Sciences.)

5. *Analysis of the Maryland Aërolite.* By Mr. George Chilton. *With an additional Notice of its physical Characters,* by Prof. Silliman.

In the *Annals* for September last, we gave Dr. Carver's account of the fall of this stone: we now extract from Silliman's Journal the following particulars of its chemical constitution and physical characters.

Results of Mr. Chilton's analysis of the Maryland aërolite: specific gravity 3·66. Twenty-five grains of the unmagnetical portion contained:

Silica	14·90
Magnesia	2·60
Lime	0·45
Oxide of iron	6·15
Oxide of nickel	0·80
Sulphur	1·27
Alumina	0·05
	26·22

The same quantity of the magnetical portion gave,

Oxide of iron	24·00
Oxide of nickel	1·25
Silica, with other earthy matter	3·46
Sulphur	A trace
	28·71

Prof. Silliman's Additional Notice of the physical Characters of the Maryland Aërolite.

As the visits of these extraordinary strangers to our planet are frequent, and their origin is not yet satisfactorily explained, it is obviously proper to register carefully all the facts respecting them, that thus we, or those who follow us, may, by and bye, be in a condition to reason correctly respecting them.

We hastened to lay before our readers the account which we received of the fall of the Maryland aërolite; but as no specimen had then been received, it was not possible to give at that time either a description or an analysis. Mr. Chilton has supplied the analysis. We add the following notice of the appearance of the stone.

An excellent specimen, for which we are indebted to Dr. Samuel D. Carver, weighs four pounds five ounces. Its dimensions are seven inches by three and four; its form is that of an irregular ovoidal protuberance, nearly flat where it was detached

from the larger mass, and bounded by irregular curves in the other parts of the surface. In all parts, except where it has been fractured, it is covered by the usual black vitreous coating, which, in this case, especially when it is viewed by a magnifier, has more lustre than is common. This coating is severed by innumerable cracks running in every direction, and communicating with each other so as to divide the surface into polygons resembling honey-comb or madrepore, and no undivided portion of the surface exceeds half an inch in diameter.

This circumstance is much less apparent upon the aërolites of Weston (1807), L'Aigle (1803), and Stannern, in Moravia (1808): it appears to have arisen from the rapid cooling of the external vitreous crust after intense ignition. It is impossible to doubt that this crust is a result of great and sudden heat. In the Maryland aërolite it is not quite so thick as the back of a common penknife, and, as in that of Weston and Stannern, it is separated by a well defined line, from the mass of the stone beneath. The mass of the stone is, on the fractured surface, of a light ash-gray colour, or perhaps more properly of a grayish-white; it is very uniform in its appearance, and not marked by that strong contrast of dark and light gray spots, which is so conspicuous in the Weston meteorolite. The fractured surface of the Maryland stone is uneven and granular, harsh and dry to the touch, and it scratches window glass decidedly, but not with great energy. To the naked eye it presents very small glistening metallic points, and a few minute globular or ovoidal bodies scattered here and there, through the mass of the stone. With a magnifier all these appearances are of course much increased. The adhesion of the small parts of the stone is so feeble that it falls to pieces with a slight blow, and exhibits an appearance almost like grains of sand. The metallic parts are conspicuous, but they are much less numerous than the earthy portions, which, when separated, are nearly white, and have a pretty high vitreous lustre, considerably resembling porcelain. They appear as if they had undergone an incipient vitrification, and as if they had been feebly agglutinated by a very intense heat. I cannot say that I observed in them, as M. Fleuriau de Bellevue did in the aërolite of Jonzac (Jour. de Phys. tom. xcii. p. 136), appearances of crystallization, although it is possible there may have been an incipient process of that kind, especially as the small parts are translucent.* The Maryland stone is highly magnetic; pieces as large as peas are readily lifted by the magnet, and that instrument takes up a large portion of the smaller fragments.

* This vitreous appearance, I believe, has not been observed before (at least as far as appears in any account that I have seen). It seems to have resulted from intense heat; the same doubtless which covered the exterior with the black crust, and the difference of the two is probably to be ascribed to the one being covered and compressed, and to the other being on the outside.

The iron is metallic, and perfectly malleable; although none of the pieces are larger than a pin's head, still they are readily extended by the hammer. The iron in the crust is glazed over, so that the eye does not perceive its metallic character, but the file instantly brightens the innumerable points, which then break through the varnish of the crust, and give it a brilliant metallic lustre at all the points where the file has uncovered the iron. The same is the fact with the Weston stone, and with that of L'Aigle, but not with that of Stannern, in Moravia; specimens of all of which, and of the meteoric iron of Pallas, of Louisiana, and of Auvergne, are now before me. The aërolites of Jonzac and of Stannern, as stated by M. Bellevue, are the only ones hitherto discovered that do not contain native iron, and do not affect the magnet; still their analysis presents a good deal of iron, which is probably in the condition of oxide.

The iron in the metallic state is very conspicuous in the Weston stone, sometimes in pieces of two inches in length; and both in this stone and in that of Maryland, it is often brilliant like the fracture of the meteoric iron of Pallas and of Louisiana.

In the analysis of the Weston stone published in 1808, I did not discover chrome, although it was afterwards announced by Mr. Warden. I have desired Mr. Chilton to reanalyse the Weston stone, and he has nearly completed the labour, the result of which may be given hereafter; but he writes that he has not been able to discover any chrome. I am not quite sure that I discover pyrites in the Maryland aërolite, although it is mentioned by Dr. Carver in his letter.

Geology.

6. *Geological Survey of the Shores of the Severn.*

The Rev. C. P. N. Wilton, FCPS. &c. has lately been engaged in making a geological survey of the shores of the Severn, in that part of its course which passes through the parish of Aure, in Gloucestershire, to an extent of about seven miles. In this examination several interesting discoveries were made, of which detailed accounts will shortly be laid before the public.

In one place, a stratum was found of a sort of carbonized wood, much resembling Bovey coal, in which occurred, disseminated in small pieces, a white substance not hitherto met with in that matrix, and which, upon examination by Mr. Brand, was found to be sulphate of barytes.

A fossil species of alcyonium was met with in blue lias—a circumstance regarded as extremely curious, when mentioned to that zealous naturalist Mr. Miller, of Bristol. (Author of the Nat. Hist. of the Crinoidea, &c.) With this were found immense specimens of cornu ammonis, &c.

Near the same spot a great number of bones were met with in

diluvial gravel; a large fragment of a gigantic stag's horn; seven fragments of immense jaw bones; and teeth in great quantities.

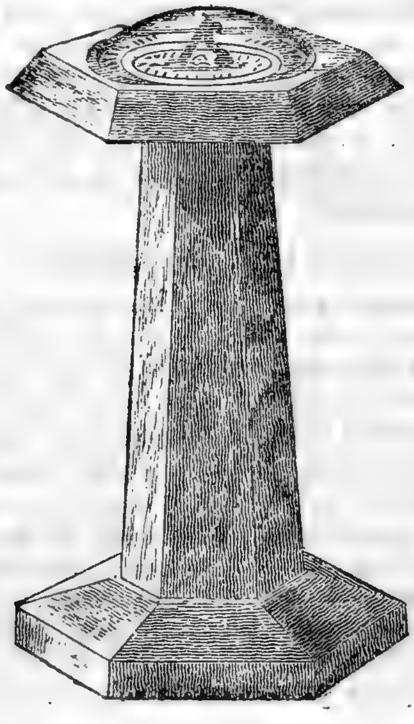
At no great distance some remains of antiquity were discovered: a sort of burying-place has been distinctly traced out: wood ashes, iron nails, and rude implements, with portions of red and black pottery, were dug up; and at about two miles distance, fragments of the same pottery were found mixed with a quantity of iron slag.

In another spot on the banks of the Severn, in a bed of clay nine feet below the surface, was dug up a sort of iron shovel, much corroded, accompanied by fragments of red pottery, and *wood in a state resembling coal.*—(Journal of Science.)

Miscellaneous.

7. *The Pantochronometer.*

An instrument has lately been presented to the public, under this name, by Messrs. C. Essex and Co. which we think calculated to be of so much utility in familiarizing the minds of young people with several important points in astronomy, that at a period when every effort is making to render scientific knowledge universally attainable, we think it worthy of particular notice in the *Annals.* The annexed figure will give a correct idea of its appearance in one of the various forms in which it is constructed.



It consists essentially of the following parts: a Sun-dial, which being affixed to a magnetic needle suspended in the usual way, in such a manner as to allow for the effect of the variation, adjusts itself in every position of the instrument, and the divisions of the hours and their fractions on which are carried on to an additional exterior circle correspondently divided; and a Fixed Circle, around the dial, on which are inscribed the names of a number of places in every quarter of the world. By this arrangement, the shadow of the gnomon, whilst it gives the time at the place of observation, also gives it for any other place required; and a series of tables contained in an explanatory work accompanying the instrument, extends its application to most places of consequence on the globe. A rule is also given for determining the longitude in degrees from the time.

We like the plan of constructing introductory pieces of apparatus like this, as they tend to habituate those who use them to instrumental observation; and we are convinced it may be much and usefully extended. Whilst thus expressing our approbation of such means of teaching the principles of science, we wish to impress the minds of those persons who may bring them forward with the necessity of taking up the subjects to be taught in their most accurate form,—with every modern correction. Popular science in this country is for the most part antiquated, and much below the standard of the present day; and this has arisen from the erroneous and mischievous idea that it is unnecessary, for such purposes, to notice the modern refinements, and sufficient to give the "broad outline," as it is called:—but they who think so, forget that the minute investigations of the present century have, in many cases, materially altered the form of that outline. We do not observe, however, any neglect of this kind in the construction of the Pantochronometer; and indeed its adjustment for the variation of the needle is an instance of the attention we recommend.

8. *French Voyage of Discovery.*

The Paris Academy of Sciences, in its sittings on Monday, received a letter from the Minister of the Marine, announcing that the corvette L'Astrolabe, Capt. Dumont de Durville, was about to sail on a voyage of discovery, and requesting the Academy to appoint a Commission to prepare such instructions as might be judged expedient. The object of the expedition is to explore certain parts of the globe, which are not sufficiently well known, and particularly the coasts of New Guinea, and those of New Zealand. A Commission, consisting of Messrs. Cuvier, Arago, Delaplace, Desfontaines, Dulong, and Aubrone de Rossel, was appointed in consequence.—*Galignani's Messenger.*

9. *Abstract of Capt. Kater's Account of the Construction and Adjustment of the new Standard of Weights and Measures of the United Kingdom of Great Britain and Ireland, lately read before the Royal Society.*

The author, after stating that the weights and measures of the United Kingdom are founded on a standard, whose length is determined by its proportion to that of a pendulum vibrating mean time in London, which has been ascertained by him to be 39·13929 inches of Sir George Shuckburgh's scale, deems it necessary, on account of the importance of the result, to consider what degree of confidence it is entitled to. For this purpose it is necessary to compare this final result with those obtained in other experiments and by different methods. Now it appears that previously to the experiments detailed in the author's paper on the subject in the Phil. Trans. for 1818, on which this result rests, another series is there mentioned, made with the same instruments, but under circumstances which occasioned their rejection, and which owing to some repairs in the instruments between the two series, which occasioned a material alteration in the distance between the knife edges, have all the weight of experiments made with a different pendulum. The result of these rejected experiments, however, differed only two ten-thousandths of an inch from that ultimately adopted.

The author next compares the lengths of the seconds pendulum at Unst and at Leith Fort, as ascertained by him with an invariable pendulum, whose vibrations had previously been determined in London, and whose length was thus known in terms of the London seconds pendulum, and as ascertained by M. Biot at the same stations by means of a variety of pendulums, and by a totally different method of observation—that of Borda. The results of this comparison are, a difference between the determinations of Mons. B. and of the author, of 0·00029 inch in excess at the former station, and 0·00015 in defect at the latter.

From this near agreement of all the results, he considers that the length of the seconds pendulum in London may be regarded as certainly known to within one ten-thousandth of an inch; while from the near agreement of the results of the French and English experiments on the length of the pendulum, he concludes that the length of the metre in parts of Sir G. Schuckburgh's scale may also be regarded as known within one ten-thousandth of an inch.

From an account recently published by Captain Sabine of his valuable experiments for the determination of the variations in length of the seconds pendulum, Capt. K. observes, doubts may be inferred of the accuracy of the method employed by him for the

observations for determining the length of the seconds pendulum in London, as well as in those which have been made with the invariable pendulum. It is asserted there, that taking a mean between the disappearances and re-appearances of the disk is a more correct method of observation than that pursued by Captain Kater, and that the intervals between the coincidences obtained, by observing the disappearances only, of the disk, would be productive of error.

In answer to this objection, the author remarks, 1st. That with respect to the convertible pendulum, or that used for determining the absolute length of the seconds pendulum, the disk was made to subtend precisely the same angle as the tail-piece of the pendulum, so that at the moment of disappearance, its centre necessarily coincided precisely with the middle of the tail-piece, and the difference between the moments of disappearance and re-appearance is rigorously nothing; an adjustment indispensable in his method of observing, when the object is to determine the *true* number of vibrations in 24 hours.

2dly. With the invariable pendulum the disk subtended a somewhat less angle than the tail-piece, so that the inferred number of vibrations in 24 hours was diminished about two-tenths of a second. But experiments with the invariable pendulum being intended to be in the strictest sense of the word comparative, this constant difference will no way affect the ultimate result. But, as the most direct way to remove any doubts which may be entertained on the subject, the author has computed from the whole of Captain Sabine's observations, the successive differences in the vibrations at the various stations visited by him, by the two methods; viz. that of employing the disappearances and re-appearances, and the disappearances alone. The results only in one instance differ so much as a tenth of a vibration, they are indifferently in excess and in defect, and the mean of their discrepancies is exactly nothing. From this he concludes that if the observations be made as nearly as possible under similar circumstances, the method of observing by disappearances alone, is productive of no perceptible error in practice, in experiments with the invariable pendulum; while in those with the convertible pendulum, the equal apparent sizes of the disk and tail-piece, preclude the possibility of any, either in practice or theory, from this cause.

The standard of Sir G. Shuckburgh having been found identical with that by Bird, in the custody of the Clerk of the House of Commons, adopted as the imperial standard unit of extension, the length of the pendulum already determined is fixed with the same degree of precision in parts of the imperial standard yard.

A repetition of Sir G. Shuckburgh's experiments on the weight of given volumes of distilled water, and a re-measure-

ment of the cube, sphere, and cylinder used by him, were found to give no material variation from his results, these being stated in terms of the mean of several standard weights kept at the House of Commons. The troy pound nearest the mean has been adopted, and declared by the legislature to be the original unit of weight under the denomination of the imperial standard troy pound.

The relation between this pound and the cubic inch of distilled water at 62° Fahr., bar. 30 in., has been ascertained by the Commissioners of Weights and Measures, who find that the latter contains 252.458 gr., each grain being the 5760th part of the standard troy pound.

The avoirdupois pound is fixed by assigning its proportion to the standard troy pound, so as to contain exactly 7000 such grains.

The imperial standard gallon is defined by stating its contents under the same circumstances of temperature and pressure, at 10 lbs. avoirdupois of distilled water, and the bushel by its containing 80 such pounds.

The author, having in compliance with a request of the Lords Commissioners of His Majesty's Treasury, undertaken to superintend the construction of, and to adjust, the principal standards to be deposited at the Exchequer, Guildhall, Dublin, and Edinburgh; Mr. Dollond was directed to prepare those of linear measure, and Mr. Bate those of weight and measure of capacity, the proper quality of metal for the latter purpose being determined by experiments instituted for the purpose. The experiments for adjusting them are then given in full detail. The troy pounds were first adjusted, and the exactness with which this operation has been performed may be appreciated from this, that the final errors of none of them exceeded 22 ten-thousandths of a grain. When brought so near, it was of course not thought necessary to attempt further correction.

The avoirdupois pounds and the weights of the gallon of water were then derived from the troy pounds, and finally adjusted, like them, by inclosing within the weight in hollows left for the purpose, wires equal to the errors ascertained to exist in them. The weights of these wires in each case is stated, so that should they by any accident be taken out and lost, they may be restored.

He next describes the method used in adjusting the gallon itself, the method of filling it exactly, and of weighing it when filled, together with the corrections depending on the circumstances of temperature and pressure under which the experiments were made. As a final result, it appears that one only of the gallons was ultimately found in error to a greater extent than six-tenths of a grain, the others having their errors less than a fourth of that quantity.

The quarts and pints being next disposed of, the author describes the balance contrived by him for weighing the bushels, which proved so delicate as to turn with a single grain when loaded with 250 lbs. in each scale. The resulting bushels when finally adjusted, were found to have all their apparent errors less than 6·56 grains of water; while the corrections for temperature and pressure only, amounted in some cases to no less than 138 grains; but this depending on the figure of the glass used to cover them, it is not to be understood that the contents of the vessels have actually been ascertained to this degree of precision.

The adjustment of the standard yards is next described; and the author concludes his paper by a summary of the results arrived at in the present inquiry respecting British weights and measures. The length (he remarks) of the pendulum vibrating seconds in London has been found in parts of the imperial standard yard, so that the value of the yard may at any time be known, having been referred to a natural standard presumed unalterable. The length of the French metre, a standard expressing a certain portion of the terrestrial meridian, has also been given in parts of the English scale. The weight of a cubic inch of distilled water has been determined in parts of the imperial troy pound, and thus the pound, if lost, may at any future age be recovered. The avoirdupois pound is now for the first time defined, and the measures of capacity are made to depend on the weight of water they contain; the imperial gallon, containing ten pounds avoirdupois of water, having been declared to be the unit, or only standard measure of capacity from which all others are to be derived. This, it is to be presumed, will tend to produce uniformity throughout the United Kingdom, by putting it in the power of every individual possessed of standard weights, to verify his measures of capacity with the utmost facility.—(Journal of Science.)

Article XIV.

NEW SCIENTIFIC BOOKS.

JUST PUBLISHED.

A Critical Inquiry into the Ancient and Modern Method of curing Diseases in the Urethra and Bladder, &c. By Jesse Foot. Eighth Edition. 6*s*.

Practical Observations on Distortions of the Spine, Chest, and Limbs; by W. Tilleard Ward, FLS. &c. 8vo. 7*s*.

Beaupré's Treatise on the Effects and Properties of Cold, with a Sketch, Historical and Medical, of the Russian Campaign; translated by J. Glendenning. 8vo. 10*s*. 6*d*.

Tiedeman's Anatomy of the Brain. 8vo. 12*s*.

Kirby and Spence's Entomology. Vols. 3 and 4. 2*l*. 2*s*.

Hall's Medical Essays. 8vo. 4*s*.

Mr. Davies's New Tables of Life Contingencies. 8vo. 10*s*. 6*d*.

Article XV.

NEW PATENTS.

F. Halliday, Ham, Surrey, for improvements in machinery to be operated upon by steam.—Dec. 9.

J. C. Dyer, Manchester, patent card manufacturer, for improvements in machinery for making wire cards for carding wool, cotton, tow, &c.; and also certain improvements on a machine for shaving and preparing leather used in making such cards.—Dec. 9.

R. Addams, Theresa Terrace, Hammersmith, for a method of propelling carriages on turnpike, rail, or other roads.—Dec. 14.

M. Ferris, Longford, Middlesex, calico printer, for improvements on presses or machinery for printing cotton and other fabrics.—Dec. 14.

J. A. Tabor, Jewin-street, Cripplegate, for means for indicating the depth of water in ships and vessels.—Dec. 14.

J. M'Curdy, Cecil-street, Strand, for improvements in generating steam.—Dec. 27.

J. Ogston and J. T. Bell, Davies-street, Berkeley-square, watchmakers, for improvements in the construction or manufacture of watches of different descriptions.—Jan. 6, 1826.

R. Evans, Bread-street, coffee merchant, for improvements in the apparatus for, and process of, distillation.—Jan. 7.

H. Houldsworth, the younger, Manchester, cotton-spinner, for improvements in machinery, for giving the taking-up or winding-on motion to spools or bobbins, and tubes or other instruments on which the roving or thread is roving, spinning, and twisting machines.—Jan. 16.

B. Newmarch, Cheltenham, for an improved method of exploding fire-arms.—Jan. 16.

J. Rothwell, Manchester, tape manufacturer, for his improved heald or harness for weaving purposes.—Jan. 16.

H. A. Koymans, Warnford-court, Throgmorton-street, merchant, for improvements in the construction and use of apparatus and works for inland navigation.—Jan. 16.

J. F. Smith, Dunston Hall, Chesterfield, for an improvement in the process of drawing, roving, spinning, and doubling wool cotton, and other fibrous substances.—Jan. 19.

W. Whitfield, Birmingham, for improvements in making or manufacturing of handles for saucepans, kettles, and other culinary vessels, and also tea-kettle handle straps, and other articles.—Jan. 19.

B. Cook, Birmingham, brass founder, for improvements in making or constructing hinges of various descriptions.—Jan. 19.

A. R. Leorent, Gottenburgh, merchant, now of King-street, Cheapside, for a method of applying steam without pressure to pans, boilers, coppers, stills, pipes, and machinery, in order to produce, transmit, and regulate various temperatures of heat in the several processes of boiling, distilling, evaporating, inspissating, drying, and warming, and also to produce power.—Jan. 19.

Sir R. Seppings, Knt. Somerset House, for an improved construction of such masts and bowsprits as are generally known by the name of made masts and made bowsprits.—Jan. 19.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1825.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
12th Mon.							
Dec. 1	E	29·80	29·17	42	26	—	40
2	N W	29·36	29·17	46	40	—	—
3	S W	29·38	29·36	45	37	—	
4	N W	29·55	29·38	39	27	—	45
5	N E	29·58	29·55	48	35	—	50
6	S	29·58	29·42	50	35	—	
7	S E	29·56	29·42	52	36	—	
8	E	29·60	29·56	43	42	—	
9	N E	29·82	29·60	45	42	—	—
10	N W	29·96	29·82	46	42	—	15
11	N W	30·02	29·96	45	35	—	—
12	N W	30·02	29·98	42	36	—	02
13	N W	29·98	29·55	52	40	—	26
14	S	29·76	29·48	46	37	—	21
15	W	29·89	29·76	56	40	—	02
16	W	29·96	29·89	58	50	—	25
17	S W	29·89	29·85	58	45	—	
18	S W	29·85	29·57	57	50	—	10
19	S W	29·62	29·57	53	42	·46	—
20	S E	29·62	29·61	52	48	—	
21	S E	29·72	29·61	53	50	—	09
22	E	30·04	29·72	54	50	—	
23	W	30·20	30·04	50	40	—	15
24	W	30·21	29·93	55	42	—	
25	S W	30·10	29·93	56	43	—	
26	S W	30·10	30·02	42	25	—	—
27	N W	30·02	29·88	36	28	—	
28	N W	29·88	29·82	40	32	—	
29	N W	29·82	29·80	36	33	—	
30	N W	29·90	29·80	38	28	—	
31	N W	29·95	29·90	38	27	·45	10
		30·21	29·17	58	25	·91	2·70

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Twelfth Month.—1. Fine. 2. Morning rainy: afternoon fine. 3. Fine. 4. Rainy. 5. Very wet morning and evening. 6—8. Fine. 9, 10. Cloudy. 11, 12. Mornings foggy. 13. Fine. 14. Rainy. 15. Fine. 16. Fine: night rainy. 17. Drizzly. 18. Fine. 19. Rainy morning. 20. Fine. 21. Cloudy. 22. Gloomy. 23. Gloomy: rain at night. 24. Fine. 25. Morning gloomy: drizzly: afternoon fine. 26. Rainy evening. 27. Fine: clear and frosty. 28. Ground covered with snow this morning: gloomy. 29. Overcast. 30. Gloomy. 31. Fine.

RESULTS.

Winds: NE, 2; E, 3; SE, 3; S, 2; SW, 6; W, 4; NW, 11.

Barometer: Mean height

For the month .... 29·755 inches.

Thermometer: Mean height

For the month .... 43·016°

Evaporation .... 0·91 in.

Rain .... 2·70

Laboratory, Stratford, First Month, 21, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1826.

Article I.

On the Going of a Clock with a Wooden Pendulum. By Col. Beaufoy, FRS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Bushey Heath, Stanmore, Feb.* 2, 1826.

In the *Annals of Philosophy* for March, 1820, and June, 1822, were published the description and going of a clock with an unvarnished pendulum of deal; the regularity of time kept with this kind of wood induced me to try the effect of a rod made of wood of greater density; for this purpose a very clear, dry, and straight-grained piece of teak was selected, and the annexed table contains the result of eighteen months' observations. By comparing this table with the former, it appears that a pendulum with a rod of *heavy* wood is, in point of accuracy, inferior to one made of *light*, the greatest daily variation of the clock with the latter being from minus 2·74″ to plus 1·48″, sum 4·52″; with the teak from minus 2·52″ to plus 9·5″, sum 12·02″; whether the exactness of the time be in proportion to the lightness of the wood will be the subject of future experiments.

I remain, Gentlemen, yours, very truly,

Mark Beaufoy

Table containing the Going of a Clock with a Teak Pendulum.

	Clock fast or slow.	Diff.	Daily rate.		Clock fast or slow.	Diff.	Daily rate.
1824.				1824.			
April 1	+0′ 01·40″			Nov. 3	+0′ 04·53″	4·15″	−1·05″
7	+0 03·00	1·60″	+0·27″	6	+0 03·43	1·10	−0·37
10	+0 03·90	0·90	+0·30	15	−0 05·92	9·35	−1·04
27	+0 16·00	12·10	+0·71	21	−0 16·20	10·28	−1·71
29	+0 17·43	1·43	+0·71	24	−0 19·72	3·52	−1·17
May 1	+0 18·36	0·93	+0·46	29	−0 24·13	4·41	−0·88
6	+0 23·15	4·79	+0·96	Dec. 1	−0 26·82	2·70	−1·35
10	+0 24·07	0·92	+0·23	3	−0 28·87	2·05	−1·02
19	+0 34·75	10·68	+1·18	8	−0 33·41	4·54	−0·91
21	+0 37·06	2·31	+1·16	10	−0 35·80	2·39	−1·20
26	+0 43·18	6·12	+1·22	12	−0 38·13	2·33	−1·16
31	+0 45·21	2·03	+0·41	16	−0 48·20	10·07	−2·52
June 1	+0 45·51	0·30	+0·31	20	−0 56·54	8·34	−2·08
4	+0 47·55	2·00	+0·67	26	−1 06·86	10·32	−1·72
7	+0 49·65	2·10	+0·70	29	−1 07·68	0·82	−0·28
9	+0 49·75	0·10	+0·05				
12	+0 51·16	1·41	+0·47	1825.			
18	+0 59·88	8·72	+1·45	Jan. 1	−1 08·53	0·85	−0·28
22	+1 02·75	2·87	+0·72	6	−1 09·60	1·07	−0·21
28	+1 06·00	3·25	+0·55	8	−1 08·14	1·46	+0·73
July 2	+1 05·73	0·27	−0·07	13	−1 04·28	3·26	+0·65
4	+1 03·89	1·84	−0·92	17	−0 59·56	5·32	+1·33
9	+0 58·04	5·85	−1·17	19	−0 56·56	3·00	+1·50
13	+0 57·94	0·10	−0·02	26	−0 46·25	10·31	+1·47
17	+0 53·43	4·51	−1·13	29	−0 38·95	7·30	+2·43
19	+0 51·60	1·83	−0·92	Feb. 2	−0 34·30	4·65	+1·16
21	+0 50·97	0·63	−0·32	4	−0 30·89	3·41	+1·70
23	+0 51·31	0·34	+0·17	8	−0 23·10	7·79	+1·95
27	+0 52·77	1·46	+0·37	12	−0 17·04	6·16	+1·54
Aug. 3	+0 53·40	0·67	+0·09	21	−0 03·85	13·19	+1·46
9	+1 09·44	16·04	+2·67	March 4	+0 18·68	22·53	+1·88
17	+1 04·91	4·53	−0·57	8	+0 27·65	8·97	+2·24
21	+1 05·15	0·24	+0·06	12	+0 33·44	5·79	+1·45
24	+1 04·16	0·99	−0·33	14	+0 35·31	1·87	+0·94
27	+1 07·35	3·19	+1·06	18	+0 42·99	7·68	+1·92
Sept. 1	+0 54·00	13·35	−2·67	21	+0 48·76	5·77	+1·92
6	+0 50·28	3·72	−0·76	24	+0 54·61	5·85	+1·95
9	+0 51·65	1·37	+0·46	31	+1 11·83	17·22	+2·46
13	+0 53·15	1·48	+0·37	April 2	+1 18·34	6·51	+3·25
16	+0 50·60	2·58	−0·84	4	+1 23·38	5·04	+2·52
18	+0 48·33	2·27	−1·13	8	+1 37·30	13·62	+3·40
22	+0 42·53	5·80	−1·45	12	+1 52·47	15·47	+3·87
25	+0 38·24	4·29	−1·43	18	+2 21·63	29·16	+4·86
28	+0 36·42	1·82	−0·61	21	+2 38·24	16·61	+5·54
Oct. 4	+0 32·58	3·84	−0·64	28	+3 14·26	36·02	+5·15
9	+0 26·88	5·70	−1·14	30	+3 23·08	8·82	+4·41
14	+0 22·37	4·51	−0·90	May 2	+3 31·60	8·52	+4·26
18	+0 21·80	0·57	−0·14	4	+3 39·46	7·86	+3·93
24	+0 14·76	7·04	−1·17	7	+3 49·26	9·80	+3·27
30	+0 08·68	6·08	−1·01	10	+3 58·27	9·01	+3·00

	Clock fast or slow.		Diff.	Daily rate.		Clock fast or slow.		Diff.	Daily rate.
1825.					1825.				
May 14	+ 4′	11·67″	13·40″	+3·35″	Aug. 1	+0′	16·97″		
17	+ 4	22·53	10·86	+3·62	6	+1	04·50	47·53″	+9·50″
22	+ 4	40·43	17·90	+3·98	14	+2	15·70	71·20	+8·90
25	+ 4	51·15	10·72	+3·57	20	+2	56·19	40·49	+6·75
28	+ 5	03·94	12·79	+4·26	23	+3	13·00	16·81	+5·60
30	+ 5	14·72	10·78	+5·39	26	+3	27·40	14·40	+4·80
June 6	+ 5	44·46	29·74	+4·25	30	+3	45·05	17·65	+4·91
15	+ 6	12·50	28·04	+3·12	Sept. 1	+3	52·54	7·49	+3·75
18	+ 6	26·85	14·35	+4·78	3	+3	58·50	5·96	+2·98
20	+ 6	38·63	11·78	+5·89	7	+4	16·63	18·13	+4·56
23	+ 6	56·94	18·31	+6·10	12	+4	39·32	22·69	+4·54
25	+ 7	09·49	12·55	+6·27	20	+5	02·15	22·83	+2·83
30	+ 7	40·99	31·50	+6·26	26	+5	10·00	7·85	+1·31
July 2	+ 7	53·43	12·44	+6·22	29	+5	13·50	3·50	+1·17
5	+ 8	08·67	15·24	+5·08	Oct. 1	+5	17·94	4·44	+2·22
10	+ 8	35·81	27·14	+5·43	8	+5	25·25	7·31	+1·04
13	+ 8	48·13	12·32	+4·11	12	+5	27·83	2·58	+0·64
15	+ 8	55·72	7·59	+3·79	16	+5	30·51	2·68	+0·67
17	+ 9	03·79	8·07	+4·03	21	+5	39·46	8·95	+1·79
20	+ 9	19·76	15·97	+5·32	27	+5	52·60	13·14	+2·19
23	+ 9	43·90	24·14	+8·05	31	+5	59·23	6·63	+1·66
25	+10	01·82	17·92	+8·97	Nov. 6	+6	10·21	10·98	+1·83
27	+10	20·57	18·75	+9·37	12	+6	25·89	15·68	+2·65
30	+10	46·85	26·28	+8·76	19	+6	44·97	19·08	+2·73
	Stopt	the	clock.		23	+6	53·46	8·49	+2·12

Article II.

Results of various Meteorological Registers for the Year 1825.

1. *For Helston, in Cornwall.* By M. P. Moyle, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Helston, Jan.* 2, 1826.

In transmitting you my Meteorological Journal for the past year, I beg to remark that it differs only from my former method of observation in having a *black bulb* thermometer placed in the direct rays of the sun; its bulb has a slight coat of black paint, into which is stuck a quantity of finely chopped black wool, and it is mounted on a box-wood scale, a foot above the slated roof of a house. It may be useful in ascertaining the solar radiation of this part of the country.

I am, Gentlemen, your obedient servant,

M. P. Moyle.

1825.	Barometer. Monthly mean.	Barometer. Maximum.	Day.	Minimum.	Day.	Rain. In inches.	Registering Thermometer. In the shade. Maximum.	Minimum.	Mean.	Monthly mean.	Black bulb in the sun, monthly mean.	Wet days.	Dry days.	Winds and Weather. E.	NE.	N.	NW.	W.	SW.	S.	SE.	Prevailing winds.
Jan.	30·2864	30·8770	9	29·3878	18	2·665	53	26	39·5	43·30	59·0	14	17	6	3	2	5	2	8	2	3	SW
Feb.	30·1087	30·5140	9	29·5541	29	2·130	52	33	42·5	42·20	59·4	12	16	4	2	6	7	4	5	0	0	NW
March.	30·1791	30·6000	20	29·1782	1	3·125	55	33	44·0	44·10	67·2	14	17	16	0	3	2	5	2	1	2	E
April.	29·9809	30·4500	2	29·1322	28	2·155	66	37	51·5	51·20	82·1	5	25	10	0	2	5	4	7	2	0	E
May	29·9000	30·3660	31	29·4687	2	3·275	70	43	56·5	45·50	82·7	13	18	5	0	3	4	4	7	4	4	SW
June	29·9741	30·3163	1	29·5717	4	1·330	77	42	59·5	59·40	91·0	9	21	7	0	3	4	5	7	1	3	SW
July	30·0808	30·3166	5	29·8590	1	0·230	86	50	68·0	67·00	101·6	1	30	5	7	9	6	0	2	0	2	N
August	29·9386	30·3825	21	29·1610	4	2·670	77	51	64·0	63·50	89·6	10	21	3	4	2	9	4	6	1	2	NW
Sept.	29·8140	30·2088	2	29·3191	18	2·100	73	48	60·5	61·60	85·8	10	20	1	0	6	4	1	5	11	2	S
Oct.	29·9838	30·4880	15	29·1840	19	4·070	67	34	50·5	51·60	75·1	18	13	1	1	3	5	6	9	3	3	SW
Nov.	29·7683	30·4000	23	28·8000	29	4·240	57	34	45·5	45·70	59·0	21	9	0	2	5	7	6	8	2	0	SW
Dec.	29·5499	30·2000	24	28·9211	7	4·420	53	28	40·5	44·70	61·2	19	12	4	2	3	4	2	9	2	5	SW
Mean of the means	29·9620	30·4266		29·2947		32·410	66·5	38·2	51·8	51·60	76·1	146	219	62	21	47	62	43	75	29	26	SW

BAROMETER: Highest, Jan. 9, Wind NE. …… 30·8770
Lowest, Nov. 29, Wind W and by S. 28·8000

REGISTERING THERMOMETER: In the sun; Highest, July 17, Wind NE. 122°
In the shade; Highest, July 19, Wind NE. 86
Lowest, Dec. 29, Wind E… 28

Note.—On the 27th of Dec. there was a slight fall of snow in the morning, followed, in the course of the day, by sleet and rain from the NW. In the night there were several showers, which appeared to have frozen as soon as they fell, as by the morning of the 28th the whole neighbourhood was covered by one sheet of hard and smooth ice; but which was again thawed in the course of the day. This frost was the only one in the whole year which was of six hours' duration.

2. *Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat.* 56° 23′ 30″. *Above the Level of the Sea* 140 *feet.*

1825.	Morning, 10 o'clock. *Mean height of* Barom.	Morning, 10 o'clock. *Mean height of* Ther.	Even., 10 o'clock. *Mean height of* Barom.	Even., 10 o'clock. *Mean height of* Ther.	Mean temp. by Six's Ther.	Depth of Rain. Inches.	No. of days. Rain or Snow.	No. of days. Fair.
Jan.	29·961	39·387	29·936	39·935	40·355	1·45	9	22
Feb.	29·912	39·928	29·893	39·250	40·071	0·95	9	19
March	29·992	41·742	29·978	40·161	41·709	1·20	10	21
April	29·854	47·300	29·835	43·600	46·700	2·40	9	21
May	29·873	51·322	29·897	47·097	50·096	2·60	13	18
June	29·785	57·566	29·764	53·000	56·500	2·50	9	21
July	30·010	63·097	30 020	58·129	62·032	0·30	5	26
Aug.	29·733	61·322	29·725	57·485	60·838	2·00	9	22
Sept.	29·715	58·600	29·701	54·866	57·600	2·35	16	14
Oct.	29·678	51·322	29·671	48·903	51·161	2·15	14	17
Nov.	29·451	41·400	29·417	39·833	41·066	2·80	9	21
Dec.	29·412	40·677	29·437	40·484	40·451	3·20	17	14
Means	29·781	49·742	29·773	46·895	49·048	23·90	129	236

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*		*Wind.*		Thermometer.	*Wind.*	
Highest,	Jan. 9	SW	30·80	June 16	SW	71°
Lowest,	Jan. 18	E	28·66	Dec. 31	W	25°

EVENING.

Highest,	Jan. 9	SW	30·75	July 30	SE	66°
Lowest,	Nov. 5	SE	28·64	Dec. 31	W	26°

Weather.	*Days.*
Fair	236
Rain or snow	129
	365

Wind.	*Times*
N and N E	9
E and S E	119
S and S W	95
W and N W	142
	365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, Dec. 31, Wind W	21°
Hottest, July 18, Wind W	80°
Mean temperature for 1825	49·048

Result of two Rain Gauges.	*Inches.*
Centre of Kinfauns Garden, about 20 feet above the level of the sea	23·90
On the Square Tower of Kinfauns Castle, 140 feet above sea level	23·45

3. *For New Malton, Yorkshire.* By Mr. J. Stockton, Cor. Mem. Met. Soc.

1825.	Barometer.					Thermom.			Winds.											Weather.			Rain.	
	Maximum.	Minimum.	Mean.	Spaces described in inches.	Number of changes.	Maximum.	Minimum.	Mean.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Var.	Brisk.	Boisterous.	Rain.	Snow.	Hail.	Inches.	Character of each period.
Jan.	30·80	29·01	30·086	8·47	18	52	25	37·161	5	1	1	0	2	12	4	6	0	2	1	4	2	0	1·62	Cloudy, dry, and temperate.
Feb.	30·45	29·21	30·032	5·82	14	51	22	36·935	2	0	0	0	8	11	3	4	0	0	2	2	2	0	1·89	Idem.
March.....	30·55	29·09	30·072	5·01	12	60	26	39·806	11	6	3	3	4	2	0	1	1	1	0	3	3	0	1·44	Cloudy, and wet at the beginning.
April......	30·50	29·35	29·943	3·70	13	64	30	47·450	5	4	1	2	0	6	5	3	4	7	1	5	0	1	1·98	Dry to the 20th, afterwards wet.
May	30·30	29·43	29·906	3·63	12	74	32	52·113	7	7	5	2	3	1	1	0	5	1	0	12	0	1	3·25	Wet and changeable.
June.	30·40	29·14	29·903	4·15	11	80	41	57·700	4	3	0	0	6	8	4	0	5	3	2	10	0	1	2·44	Warm, and chiefly dry.
July.	30·29	29·74	29·103	2·26	11	88	42	63·048	14	6	0	1	2	3	4	1	0	2	0	1	0	0	2·42	Clear, hot, and dry.
August....	30·26	29·12	29·838	4·30	10	84	42	61·000	6	2	3	1	3	6	7	1	2	4	2	11	0	0	4·34	Hot and wet.
Sept.	30·33	29·40	29·790	3·87	14	75	41	59·333	4	2	1	6	5	5	2	2	3	5	2	7	0	0	1·39	Fine and warm.
Oct.	30·30	28·91	29·830	6·27	10	66	29	46·500	1	0	0	3	4	16	4	3	0	6	3	11	1	0	3·12	Wet and changeable.
Nov.......	30·15	28·55	29·577	9·02	18	54	25	38·500	3	1	0	0	7	8	6	5	0	7	6	15	2	1	3·20	Wet, stormy, and changeable.
Dec.	29·84	28·80	29·457	5·54	13	51	18	38·516	5	4	0	1	5	11	0	4	1	3	3	11	2	0	3·28	Wet, damp, and cloudy.
Annual means, &c.	30·80	28·55	29·878	64·04	156	88	18	48·171	67	36	14	19	49	89	40	30	21	41	22	92	12	4	27·37	

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Jan. 9, Wind N	30·800
Lowest observation, Nov. 3, Wind NW.	28·550
Range of the mercury	2·250
Mean annual barometrical pressure	29·878
Spaces described by the different oscillations	64·040
Total number of changes in the year	156·000

Six's Thermometer.

Highest observation, July 18, Wind N.	88·000°
Lowest observation, Dec. 31, Wind N	18·000
Range of the mercury in the thermometer.	70·000
Mean annual temperature	48·171

Winds.

North.	67
North-east.	36
East	14
South-east.	19
South.	49
South-west	89
West	40
North-west	30
Variable.	21
Brisk	42
Boisterous.	22

Rain, &c.

	Inches.
Greatest quantity in December	3·280
Least quantity in July	0·420
Total amount for the year	27·370
Days of rain	92·000
Days of hail	4·000
Days of snow	12·000

New Malton, Jan. 6, 1826.

ARTICLE III.

On the Solutions of the Function $\psi^z x$, and their Limitations. By Mr. W. G. Horner.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bath, Feb.* 11, 1826.

IN pursuance of a design long entertained, I request the favour of your inserting in the *Annals* the annexed investigation of the most useful properties of the formula $\psi^z x$, or $\phi^{-1} f^z \phi x$, when $f^z x = \frac{a_z + b_z x}{c_z + d_z x}$. Since the appearance of the Ladies' Diary for 1821, containing a perfectly general solution of this equation, I had hoped that my hastily written paper of 1817 would not be quoted as the standard of my views on the subject; but, very possibly, the succinct statement in the Diary may have been overlooked by some of your mathematical readers. They will, therefore, I conceive, be gratified with the present republication of the same theorem, investigated in a rigorous, yet simple and perspicuous manner, and supported by so much of unambitious elucidation as will suffice to convey correct ideas both of the extent and the limitations which are proper to it when applied to *ordinal,* and particularly to *periodical* functions. On the latter subject, Mr. Herapath will perceive that a statement of mine, which he has controverted, is correct, and that an essential distinction exists between $\psi^2 x = x$, or the second order, and those superior to it. The foundation upon which correct theories are to be established, cannot be too cautiously laid. I am, yours, very truly,

W. G. HORNER.

1. Leaving the operations ϕ and ϕ^{-1}, which are requisite for giving the utmost extension to our conclusions, to be supplied by the reader, I proceed directly to the formula $f^z x = \frac{a_z + b_z x}{c_z + d_z x}$. Here a_z, b_z, &c. represent certain functions of a variable z, whose mutual relations to it and to each other, it is our business to determine. It is a well-known property of this formula, and for the application of which, in the first instance, I believe, we are indebted to Mr. Babbage, that, regarded as a function of x, it always reproduces formulæ similar to itself. An important consequence of this remark is, that x can never interfere in the functions of z, if it be absent from a, b, c, d, at the case $f x = \frac{a + b x}{c + d x}$, assumed as the origin of the functions; and, therefore, our reasonings, whether they regard $f^z x$ as a function of z, or of x, cannot clash with each other.

2. On the latter of the two hypotheses, $f^{z+1} x$ being equal both to $f f^z x$ and $f^z f x$, we obtain by actual substitution, the following pairs of equal values:

$$a_{z+1} = a_z b + a c_z = a b_z + a_z c \dots\dots\dots\dots (1)$$
$$b_{z+1} = b b_z + a d_z = b b_z + a_z d \dots\dots\dots\dots (2)$$
$$c_{z+1} = c c_z + a_z d = c c_z + a d_z \dots\dots\dots\dots (3)$$
$$d_{z+1} = b_z d + c d_z = b d_z + c_z d \dots\dots\dots\dots (4)$$

From (2) or (3), $a d_z = a_z d$. Assume, therefore,

$$a_z = a s_z \therefore d_z = d s_z \dots\dots\dots\dots (5)$$

Consequently $a_{z+1} = a s_{z+1}$ and $d_{z+1} = d s_{z+1}$

Substitute these four values in either (1) or (4), and we have

$$s_{z+1} = b s_z + c_z = b_z + c s_z \dots\dots\dots\dots (6)$$

Collecting the results of (5) and (6), we now have

$$\left.\begin{array}{ll} a_z = a s_z, & b_z = s_{z+1} - c s_z \dots\dots\dots\dots \\ d_z = d s_z, & c_z = s_{z+1} - b s_z \dots\dots\dots\dots \end{array}\right\} (7)$$

and consequently,

$$f^z x = \frac{a s_z + (s_{z+1} - c s_z) x}{(s_{z+1} - b s^z) + d s^z x} \dots\dots\dots\dots (8)$$

or, in a form better adapted for simplifying the results, put $q_z = \frac{s_{z+1}}{s_z}$, which gives

$$f^z x = \frac{a + (q_z - c) x}{(q^z - b) + d x} \dots\dots\dots\dots (9)$$

3. The solution, we see, hinges entirely on the quantities s; whose values may be found as follows. Comparing together (2), (3), (7), we find

$$b_{z+1} = b s_{z+1} - (b c - a d) s_z$$
$$c_{z+1} = c s_{z+1} - (b c - a d) s_z$$

and if these be substituted in $s_{z+2} = b s_{z+1} + c_{z+1}$, or $b_{z+1} + c s_{z+1}$, which is in the next gradation to (6), the result is,

$$s_{z+2} = (b + c) s_{z+1} - (b c - a d) s_z \dots\dots (10)$$

so that the quantities s in question form a recurring series, whose scale of relation is $+ (b + c)$, $- (b c - a d)$.

Now we already perceive that the terms of this scale, which for conciseness we shall denote by A and $-$ B, are not altogether arbitrary. The purpose of a *general solution* will be defeated, for example, if either of them vanishes. For, first, let $b c - a d = 0$. Then $s_{z+2} = (b + c) s_{z+1}$, which produces $q_z = b + c$, a constant quantity, and, therefore $f^z x = f x$, a constant quantity. In fact, the datum itself gives

$$f x = \frac{a}{c}, \textit{ constant} \dots\dots\dots\dots (11)$$

Secondly. Let $b + c = o$. Then $s_{z+2} = (a\,d - b\,c)\,s_z$, whence $q_{z+2} = q_z$, and therefore $f^{z+2}\,x = f^z\,x$, and $f^2\,x = x$. Hence the solution of this equation is

$$f\,x = \frac{a - c\,x}{c + d\,x} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (12)$$

4. To proceed then with the general inquiry, in which both A and B are effective quantities, equation (10) transposed becomes

$$s_{z+2} - A\,s_{z+1} + B\,s_z = 0. \quad \ldots\ldots\ldots\ldots \quad (13)$$

and, by the theory of equations, this condition is satisfied when

$$s_z = \alpha\,r^z + \beta\,\rho^z \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (14)$$

r, ρ being the roots of the equation

$$y^2 - A\,y + B = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (15)$$

α and β constant quantities whose values remain to be determined, and z any quantity whatever. In fact, direct substitution produces

$$\alpha\,r^z\,(r^2 - A\,r + B) + \beta\,\rho^z\,(\rho^2 - A\,\rho + B)$$

which is necessarily $= 0$, because each of the parenthetic factors is so.

Now when $z = 1$, we have, by (1) compared with (5), $a\,s_2 = a\,b + a\,c$, giving $s_2 = b + c = A = r + \rho$; and by (5) alone $s_1 = 1$. Hence the equations

$$\left.\begin{array}{l} s_2 = \alpha\,r^2 + \beta\,\rho^2 = r + \rho \ldots\ldots\ldots\ldots \\ s_1 = \alpha\,r + \rho = 1 \ldots\ldots\ldots\ldots\ldots\ldots \end{array}\right\} (16)$$

which yield by elimination, $\alpha = -\beta = \frac{1}{r - \rho}$, and consequently $s_0 = \alpha + \beta = 0$. And these results are confirmed by making $z = 0$ in equation (13), which thus becomes, as it ought,

$$s_2 - A\,s_1 + B\,s_0 = 0$$

The correct solution, therefore, is,

$$s_z = \frac{r^z - \rho^z}{r - \rho} \therefore q_z = \frac{r^{z+1} - \rho^{z+1}}{r^z - \rho^z} \quad \ldots\ldots\ldots\ldots \quad (17)$$

This solution, detached from the investigation, but succinctly demonstrated, according to Diarian usage, has been more than five years before the public, and is the most extensive, perhaps, that can be obtained. It remains for me to show its application to some general cases.

5. When r, ρ, are *imaginary*, we assume, as usual in quadratic equations of that description,

$$\cos.\,\vartheta = \frac{A}{2\sqrt{B}} = \frac{b + c}{2\sqrt{b\,c - a\,d}} \quad \ldots\ldots\ldots\ldots \quad (18)$$

which leads to $r, \rho = (\cos.\,\vartheta \pm \sqrt{-1}\,\sin.\,\vartheta)\sqrt{B}$, and $r^z, \rho^z = (\cos.\,z\,\vartheta \pm \sqrt{-1}\,\sin.\,z\,\vartheta)\sqrt{B^z}$.

Whence

$$s_z = \frac{\sin. z\vartheta}{\sin. \vartheta} \sqrt{B^{z-1}} = \frac{\sin. z\vartheta . A^{n-1}}{\sin. \vartheta (2\cos. \vartheta)^{n-1}} \ldots\ldots (19)$$

and this again produces

$$q_z = \frac{\sin. (z+1)\vartheta . \sqrt{B}}{\sin. z\vartheta} = \frac{\sin. (z+1)\vartheta . A}{\sin. z\vartheta . 2\cos. \vartheta}$$

The latter expression, in a more convenient form, becomes

$$q_z = \frac{\sin. (z+1)\vartheta . (b+c)}{\sin. (z+1)\vartheta + \sin. (z-1)\vartheta} \ldots\ldots\ldots\ldots\ldots (20)$$

In the mean time, the original assumption in equation (18), when resolved produces

$$d = \frac{-(b^2 - 2\cos. 2\vartheta . bc + c^2)}{(2 + 2\cos. 2\vartheta)\, a} \ldots\ldots\ldots\ldots\ldots (21)$$

But there is another quantity, which we may denote by p_z, and which is connected to the rest by the equations

$$p_z + q_z = r + \rho, \text{ and } p_z\, q_{z-1} = r\rho \ldots\ldots\ldots (22)$$

which in the case we are now examining becomes

$$p_z = \frac{\sin. (z-1)\vartheta . (b+c)}{\sin. (z+1)\vartheta + \sin. (z-1)\vartheta} \ldots\ldots\ldots\ldots\ldots (23)$$

If these values are substituted in the formulæ

$$f^z x = \frac{a + (q_z - c) x}{(q_z - b) + d x} = \frac{a + (b - p_z) x}{(c - p_z) + d x} \ldots\ldots\ldots (24)$$

which are readily seen to be convertible expressions, though one of them may occasionally be more convenient than the other, they complete the solution in the case of imaginary roots, or $4B > A^2$.

6. From these *general* formulæ, the *periodic* species are readily derived, and in a more natural and perspicuous manner than from the independent trains of reasoning which have usually been employed.

The general form, then, of the *periodic* species concerned in this inquiry, being $\psi^n x = x$, which flows from $f^n x = x$, it is obvious from Equation 8, that the general condition of solution is

$$s_n = 0, \text{ i. e. } \frac{r^n - \rho^n}{r - \rho} = 0$$

Here, if $r - \rho = 0$, s_n is $= n r^{n-1}$ $\therefore s_0 = 0$, which leads to the truism $f^0 x = x$, and is no solution.

If $r + \rho = 0$, which is a divisor of s_n only when n is an even number, we have $r + \rho = A = b + c = 0$; and, therefore,

$$f x = \frac{a - c x}{c + d x}$$

which, as we have already seen (Art. 3), is the *proper* solution of $f^2 x = x$. It is but incidentally a solution in the general event of $n = 2m$, on a well-known principle common to all the cases $n = m m_r$

Provided r and ρ be *real* quantities, the case admits no other periodical solution besides this. For $r^n - \rho^n$ has no real binomial divisors but $r - \rho$ and $r + \rho$, and its trinomial divisors are all of the form $r^2 - 2 \cos. \vartheta \,. r \rho + \rho^2$, which in this case is necessarily > 0.

It is true, that since r and ρ, as the consideration of the hypothesis $r - \rho = 0$ has proved, are always unequal, the following conditions will obtain in the general case of real roots; viz. if we take r to be the greater root, and n infinite, we shall have $q_\infty = r$ constant, and thence

$$f^\infty x = \frac{a + (r - c)\, x}{(r - b) + d\, x} = \frac{a}{r - b} \ \ldots\ldots\ldots\ldots\ldots\ldots \ (25)$$

a constant quantity. To this form then, which must be called *oscillating* rather than *periodical*, the condition of real roots ultimately approaches.

7. The instance of a ball impelled through either focus of an elliptical table, and continually reflected from the rim, affords a practical exemplification of this ultimate tendency to simple oscillation; for after an infinite number of reflexions, it will vibrate continually in the transverse axis. The analytical expression of the facts would lead to some unobserved properties of the ellipse, if this were the place for examining into them. It is as follows: let S, s, represent the focal sections of the transverse axis, e the excentricity, and x the half difference between the distances of any point of impact from the foci; then the similar half difference at the nth subsequent impact will be

$$f^n x = \frac{(S^{2n} - s\ \ \ )\, e + (S^{2n} + s^{2n})\, x}{(S^{2n} + s^{2n})\, e + (S^{2n} - s^{2n})\, x} \times e. \ \ldots\ldots \ (26)$$

which continually approximates toward the constant value e. (See L. D. *ubi supra.*)

8. After these partial or questionable instances are dismissed, it appears that *imaginary* values of r, ρ, are essential to the periodic character. The conditional equations (19), therefore, are,

$$s_n = \frac{\sin. n \vartheta}{\sin. \vartheta} \sqrt{B}^{\,n-1} = \frac{\sin. n \vartheta}{\sin. \vartheta} \cdot \left(\frac{A}{2 \cos. \vartheta}\right)^{n-1} = 0 \ \ldots\ldots \ (27)$$

Assume $\vartheta = \frac{k \pi}{n}$, and these conditions are satisfied, provided k be an integer, and $2k$ not divisible by n.

The reason of this restriction is sufficiently obvious; for, putting $2k = mn$, the formulæ (27) become, when m is even,

$$s_n = n\,(\cos. \tfrac{1}{2} m \pi \,. \sqrt{B})^{n-1} = n\,(\tfrac{1}{2} A)^{n-1}$$

expressions which, n being arbitrary, can only vanish in consequence of the simultaneous non-existence of A and B, and $\therefore$ of r and ρ, and in short of the entire hypothesis. When m is odd,

we have $A = 2 \cos. \frac{1}{2} m \pi \, . \, \surd B = 0 \therefore b + c = 0$, or $b = - c$. But, when $b = - c$, equation 21 gives $d = \frac{-c^2}{a} \therefore - c^2 - a d = 0$; that is $B = 0$; so that here also A and B vanish simultaneously.*

(*To be continued.*)

Article IV.

On the Habits and Food of the Stickleback.†

In volume 3 of the Journal of Science, p. 74, Mr. Ramage, of Aberdeen, has given an account of a Stickleback, which was taken alive with a leech "fully as large as the stickleback itself" in its intestines. The leech "in a few minutes" was protruded by the anal opening, and crawled on Mr. Ramage's hand; but "the stickleback died almost immediately after giving birth to the strange offspring, and the leech survived it only about twelve hours." The appearance and motion of the leech, it is added, "corresponded in every respect with those of the common leech, excepting that the colour was entirely white." The theory offered to account for this fact is, "that the leech was lodged in the small gut, and most probably had been swallowed by the stickleback for food when of a small size, and had grown to its present dimensions in the stickleback's belly, after having been swallowed." The leech and the stickleback were transmitted to the Museum of the Royal Society of Edinburgh.

Upon this detail it may be remarked, that the circumstance of a stickleback swallowing a leech is no uncommon one, for young leeches seem to be the favourite food of the three-spined stickleback, *Gasterosteus aculeatus, Lin.* My boys had several sticklebacks alive for some months during the last summer, and fed them at first with earth-worms, maggots, and occasionally the small house fly, which, however, did not seem to be relished. Afterwards, at my suggestion, young leeches were brought from the ditch, in which the sticklebacks were caught, as being more likely with the larvæ of aquatic insects, to form part of their natural supply, than the food which was submitted to their

* The fallacy of Mr. Herapath's attempt to prove $k = \frac{1}{2} n$ to be admissible, lies in his erroneous assumption of the "mutual independence" of the numerator and denominator in the value of d in this case. Even in the form $\frac{(b + c)^2}{2 a (1 + \cos. \pi)}$ which he employs, the very condition of simultaneous evanescence marks their dependence on each other. For $b + c = o$, gives $b^2 + c^2 = - 2 b c \therefore (b + c)^2 = 2 b c - 2 b c$ at the same time that the denominator becomes $2 a - 2 a$; which conducts to the same result as I have given.

† Edinburgh Journal of Science.

choice. These were found to be preferred to all other aliment, and for a month at least they had scarcely any other food. The species of leeches procured were the *Hirudo sanguisuga,* the *H. vulgaris,* and the *H. complanata.* To ascertain what size of leech would be swallowed, a male stickleback, of about an inch and three-quarters in length, was selected, and put in a large tumbler on a mantel-piece, where its mode of attacking and devouring its prey formed a source of amusement to the children for weeks.

On putting the leeches into the water, the stickleback darted round the tumbler with lively motions, till it found a leech detached, and in a proper situation for being seized. When the leech was very small, say about half an inch in length, it was often swallowed at once before it reached the bottom of the vessel; but when a larger one, about an inch, or an inch and a half in length, in its expanded state, was put in, and had fastened itself by its mouth to the glass, the efforts of the stickleback to seize and tear it from its hold were incessant, and never failed to succeed. It darted at the loose extremity, or when both ends were fastened at the curve in its middle, seized it in its mouth, rose to near the surface, and after a hearty shake (such as a dog would give a rat) let it drop. The leech, who evidently wished to avoid its enemy, upon its release again attached itself by its mouth to the glass; but again and again the attack was repeated till the poor leech became exhausted, and ceased to attempt holding itself by its disc. The stickleback then seized it by the head in a proper position for swallowing, and after a few gulps the leech disappeared. The *H. complanata,* being of an ovate form, and having a hard skin, was not attacked, unless when very young, and scarcely two or three lines in length;* and leeches of the other species, when pretty well grown, or longer than himself when expanded, were killed in the manner above-mentioned, but not swallowed. In one of his attempts to seize a leech, the stickleback having got it by the tail, the animal curled back, and fixed its disc upon its snout. The efforts of the stickleback to rid himself of this incumbrance were amusing. He let go his hold of the leech, which then

* It may be mentioned as a curious instance of the wonderful arrangements of nature in securing the continuance of species, that the young of the *H. complanata,* which I have generally found attached to aquatic plants, were, in one instance which fell under my notice, affixed to the under surface of the parent leech. This animal which, unlike most of its congeners, never swims, had fastened itself to the side of the glass, and three young ones, about a line in diameter, were thus exhibited to view in a most interesting light for an animal so low in the scale of existence. Thus protected, there was nothing to fear from the attacks of the stickleback, or other enemies. They moved occasionally on the disc of the mother, and it is conjectured might remain in that situation, until they had attained such a size as to render further care on the part of the parent unnecessary. To convince myself that this protection was requisite, I detached one with the point of a knife, which was instantly devoured by the stickleback. The young *H. complanata,* from its transparency, forms a beautiful object for the microscope.

hung over his mouth, and darting to the bottom and sides of the glass with all his strength, endeavoured to rub off this tantalizing morsel. This lasted for nearly a minute, when at last he got rid of the leech by rubbing his back upon the bottom of the vessel. The leech, perfectly aware of the company he was in, no sooner loosed his hold than he attempted to wriggle away from his devourer, but before he had reached midway up the tumbler, the stickleback had turned, and finished the contest by swallowing him up.

This voracious little fish not only preys upon the young of the leech, but sometimes devours the fry of its own species. In two or three instances, when leeches had not been procured, a young stickleback, about half an inch long, was dropped into the glass, and instantly swallowed. On other occasions, when some of the larger size were put in along with him, he contented himself with killing them. Perhaps the spines of these larger fish, which are erected when in danger, and upon the death of the animal, were too strong for the texture of his throat. In the ponds and ditches where sticklebacks occur, the young fry will always be found to seek protection in the shallowest parts of the water from their full-grown enemies. Our stickleback, at another time, when two minnows, much larger than himself, had been put in to keep him company, attacked them with fury. They fled from his bite in evident dismay; and one of them, finding no other means of escape, fairly leaped out of the vessel. Even a female of his own species was not better treated by this ungallant tyrant, who allowed no stranger to enter his domain with impunity.

The young of the leech being thus, it is conceived, a frequent food of the stickleback, it is not marvellous that such a little devourer should occasionally gorge himself by swallowing a leech of large dimensions for the capacity of his stomach. That this was the case of Mr. Ramage's stickleback seems evident from the situation in which it was found, near the surface of the water, and the facility with which it was caught. Leeches possess the power of contracting and expanding themselves to a great degree; and it is not in the least surprising, that, when released from pressure by the death of the stickleback, and swelled by liquid, Mr. Ramage's leech should appear to be larger than the animal that had swallowed it. That it could have lived in the stomach of the stickleback from the period when it was very young till it attained the size mentioned by Mr. Ramage, is very improbable. From the circumstance of sticklebacks feeding on leeches with avidity, it may be inferred, that nature has provided them with the means of digesting this species of aliment; and the fact of their being fed for weeks on leeches alone, and the usual processes of digestion and excretion going on, raises this inference to absolute certainty. That an

animal so tenacious of life as the leech, should, shortly after being swallowed, be found alive in the intestines of the stickleback, does not, therefore, appear wonderful, and that the stickleback should have died when "a few minutes" out of the water, and in the hands of a child, is still less so. The wonder would have been, had it continued to exist in an element so foreign to its nature, independent altogether of the danger of leech-birth in the hands of such assistants.

ARTICLE V.

On the Flame of a Candle. By Mr. John B. Longmire.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

A LIGHTED candle, being a very interesting specimen of combustion, I have added a trifle to our knowledge of it, in the following observations, which I have the pleasure to send for insertion in the *Annals of Philosophy.*

By truncating the flame of a candle, as was first ingeniously done by Mr. G. Oswald Sym, a cross section of the cone of flame is obtained; and it appears a dark disk, surrounded by a luminous ring. But in this case, the inflammable material, which should form the upper part of the flame, is forced through the meshes of fine gauze, and being, in the truncation of a common candle, the vapour of oil and dissipated wick, partly decomposed, and cooled down by contact with cold iron into an oily smoke, it obstructs the view into the interior of the undisturbed flame.

On commencing some experiments on the flame of a candle, I had not any wire gauze with me, so I made a temporary instrument with some coarse wire, which, happening to have only two oblong meshes, divided the flame at the top of the cone; and, on sinking it a little, the flame and some smoke continued to pass upwards on the outside of the exterior barrs. In consequence, I saw down the interior to the bottom of the blue flame. The wick is black, except where in contact with flame. A space is observable all round the wick, between it and the blue flame. The whole of the interior, except the wick, is a light transparent medium.

The vapour of melted tallow issues mostly from the low part of the black wick, where the heat is least in it, and the melted tallow, or warm oil, greatly prevails there.

The oil from this part of the wick has a blue flame. When tallow is touched with red-hot iron, it burns with a white flame; but so soon as the iron has cooled below red heat, blue flame commences. We thence see the reason why blue flame is

below the white flame in a candle. Blue flame is characteristic of oily ingredients, in vegetable, animal, and coaly inflammables, as cotton, paper, coals, &c. Thus if the point of a penknife blade is put through the blue flame, it returns coated with condensed oil. But oil may be drawn from any part of both flames by holding the surface of a polished pair of snuffers near them. In this case, they grow dim with mist, that, on being rubbed with the finger, passes into small globules of oil. This is a plain indication that the interior of the flame is chiefly filled with the vapour of oil; and that a part of it is constantly escaping through the flame. Some of the evaporated oil is probably oxidated in the faintly luminous medium which surrounds the cone of flame; at least this conclusion is more apparent from the mixture of blue in the low part of that medium.

The interposition of the volatilized parts of a burning candle, between the wick and air, transfers the power of generating flame from the wick to the surface of the cone of ascending volatilized matter; and hence points out the origin of flame in bituminous inflammables. The ready oxidation of their volatilized parts is represented by flame, the less rapid oxidation of their more fixed or carbonaceous parts by the pervasion of their bodies with red heat. Next the surface only, can that process (oxidation) act on the former, while the air can pass into the porous texture of the latter, and oxidate in every part at the same time. Thus flame and red heat both represent oxidation with previous decomposition.

Article VI.

Astronomical Observations, 1826. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Jan. 2. Immersion of Jupiter's first satellite....................	16h	48′	37″	Mean Time at Bushey.
	16	49	58	Mean Time at Greenwich.
Jan. 11. Immersion of Jupiter's first satellite....................	13	10	22	Mean Time at Bushey.
	13	11	43	Mean Time at Greenwich.

Article VII.

Memoir on the Expediency of surveying the Indian Archipelago.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

Amongst the many voyages of discovery and survey by which the last sixty years have been distinguished, it is not a little surprising that some of the most interesting portions of the globe should still remain unexplored. So minute and extensive have been the researches of our navigators in every other direction, that no one will be at a loss to recognize in the exception here figured, the Islands of the Indian Archipelago. Yes, notwithstanding the mighty things that have been done, this widely spreading cluster may, in many important particulars, be denominated a *terra incognita*—one of the brightest regions on the face of the earth, occupying an immense space of insular surface, and, therefore, easily accessible to our power, is, in all that regards intimate and authentic information, almost unknown to us. Not a desolate islet—not a promontory, creek, anchorage, rock, reef, or sandbank, throughout the whole expanse of the South Sea, but is laid down with an accuracy that shames the charts of many important and dangerous parts of our own coasts at home, while I question whether there be extant a scientific plan of half a dozen harbours (I except some of the more frequented ports in Sumatra and Java) of the hundreds of large and beautiful islands that garnish the coasts of further India—countries abounding in all that can administer to the wants, the comforts, and the luxuries of man.

Such ignorance is more easily explicable than it is creditable to that spirit of enterprize for which this country has, in modern times, been sufficiently conspicuous, and which has, especially of late years, been exerted upon principles and for purposes honourable to human nature.

An entirely new and glorious epoch in the history of geographical and hydrographical discovery signalized the reign of the first naturalized British monarch of the Brunswick line, and shed on it what will probably constitute its brightest lustre in the eyes of posterity; for *then* commenced that series of undertakings which had for their object not the private emolument of individuals, or of companies—not even the acquisition of national wealth, but were intended for the benefit of mankind at large. Thus while other nations boast of their wars and their conquests, we may honestly exult in the far higher renown of having carried the noble and useful science of navigation to a point of perfec-

tion unequalled, and of having furnished the groundwork of commerce, and its concomitant blessing, civilization, for generations yet unborn.

Until the year 1764 every enterprize had been set on foot for private profit, or for predatory or warlike aggression. Unoffending people were frequently invaded, insulted, subdued, and oppressed, and hostile retaliation involved the good with the guilty. It was reserved for Britain to be the first to renounce such debasing motives of conduct, and to set an example which the nations of the world might attempt to emulate, but could not hope to excel. The moment we set sail from Spithead with Commodore Byron, in the Dolphin (the very names and associations are endearingly oceanic) the sea becomes British classic ground, and this without any straining after metaphor; for one of the most patriotic of our poets, whose muse is identified with our naval glory, has said, with a felicitousness of diction unrivalled—

> "Her march is o'er the mountain wave,
> Her home is on the deep."

All the principles of nautical adventure from that hour assumed a new and more dignified character. Objects the most interesting to society were pursued to the sole end that society might reap the advantage. An invaluable accumulation of natural and scientific knowledge has been the result.

To those who rejoice in the moral, as well as in the physical and political strength of the country, another effect not less important, though, perhaps, not quite so evident, will appear to have sprung out of the enlightened zeal, liberality, and publicity, with which all these expeditions have been conducted. All has been fair and above board; nothing has been kept back. From the number and competency of the individuals selected for these enterprizes, and from the permission granted to all to keep journals, the most effectual checks to falsehood, the highest incitements to truth, and the fullest guarantee of accuracy, have been thoroughly ensured to the utmost limit that human knowledge admits of. By this means there has been created a greater demand, or rather a vehement longing after truth, and as a happy consequence, the standard of value of that precious commodity has been greatly enhanced.

Prior to the brilliant era in our history to which I have alluded, our information respecting distant countries was of a very dubious and defective kind, depending entirely on the honesty and ability of individual travellers, and at best was taken with a large abatement of confidence. Certain general indefinite notions were acquired from the details of the older navigators, but when it was wished to ascertain positive facts, an intuitive distrust was felt that nothing could overcome. So much of the vague and the marvellous entered into their histories, that it was

often impossible to distinguish what was true from what was false. The mind had no resting place. Not to go further back than Dampier, the most intelligent, correct, and instructive voyager, considering his rank and station, that any country has produced, it is well known that his accounts, until confirmed, as they have been, to the minutest point by subsequent accredited authorities, were regarded as little better than the tales of a Buccanneer. The same may be said of Lionel, Wafer, Woods, Rogers, and others of the like stamp. Even Anson's voyage, though performed by order of the government, has not been given to the world with all the marks of authenticity about it that could be wished. Bruce, the Abyssinian, was set down as a mendacious fabulist, till lately that justice has been done to his veracity. Hearne's account of the Copper Mine River was not implicitly believed until the disastrous but wonderful Journey of Capt. Franklin to the Shores of the Polar Sea demonstrated its general truth. Even the Narrative of Mackenzie, though bordering nearly on truth, remains to this day unauthenticated, except by the reports of the fur traders; nor will scepticism on some points be wholly set at rest, till the same intrepid and indefatigable officer dispose of it as he did of the other. Much uncertainty prevailed respecting the extent and direction, and even the existence of Baffin's Bay as a *bay*, and of Lancaster Sound, &c. and it is but within the last seven years that we have witnessed the final adjustment of all those important questions, and this, be it remembered, has been effected only after repeated *public* expeditions, under the sanction of government, and upon the solemn and conclusive guarantee of national good faith. Compared with the private individual accounts of which mention has been made, how different is the state of our knowledge derived from those invaluable public sources. All the results have, to the utmost attainable degree, the attributes of moral and physical truth.

A lofty point of elevation has thus been gained by the country in regard to its present character, but more especially in the relation which that character bears to posterity. Of this we may be satisfied, if indeed we require any assurance, by reflecting how much the want is felt of some standard by which to judge of the actual truth and condition of many things belonging to antiquity, as these are handed down to us by individuals, whether poets or historians. How difficult to get at realities! Often not till after laborious collating of obscure hints and phrases, and then only by the help of grammatical subtleties, strained analogies, and constructive interpretations. If, for example, instead of the scanty and imperfect notices of certain tribes of people afforded by Greek and Roman writers (however worthy of credence, they possessed few means of informing themselves correctly), we could have the history of an expedi-

tion sent out by the government of Athens or of Rome, at the commonwealth's expense, in the view and on the responsibility of the public, how much higher would our gratification be, and how much more satisfactory our information. Extending this principle a little further, may I not be pardoned for suggesting that it might be an object not unworthy of a liberal and fostering government, to institute a series of accredited expeditions into every country (even of Europe) of which our knowledge is defective, and not leave the investigation to casual explorers whose statements, under every advantage, are liable to error. A body of general and statistical knowledge would thus be obtained, available for every useful purpose. This is a view of the subject that I am desirous of urging strongly, being convinced of its beneficial influence in promoting principles and habits which tend to exalt us as a people. Indeed Government has in some sort created the desideratum, which, I trust, will not be wanting in all practical endeavours to supply.

But to return.—It does not seem very difficult to explain, or, at all events, to state some of the facts and circumstances which may have conspired to make us acquainted with the islands of the Southern, Pacific, and Atlantic, and to conceal from our more immediate view those of the Indian Archipelago. The latter were known to exist. Curiosity as to original discovery was, therefore, laid asleep. The India Company had, as it were, begirt the coasts of the East with a belt of iron that no private enterprize dared to penetrate. The influence of monopoly has since continued to shut out the British people from all participation in the advantages of trade. Though the Archipelagan range did not lie within the more immediate jurisdiction of the Company, yet what amounted to a prohibition of intercourse was imposed by the regulations of this chartered body. So much of the productions of the East as sufficed for preventing the different articles from falling too low in the market, have been doled out to the country, that India Bonds might maintain their price, and India Directors their place. While every European power that can send a ship out may procure the (now) prime necessary of life *tea* for a trivial consideration, the people of Great Britain must pay an exorbitant price for the sake of supporting in affluence a copartnery to which they owe not one obligation.

All these circumstances threw insurmountable obstacles in the way of private mercantile adventure or speculation, and to this hour have closed, almost hermetically, all the avenues by which the Indian Seas, and the innumerable resources pent up within them, might even be approached. Nor can I help presuming that Government itself must often have been deterred, or at least discouraged, from pushing forward any liberal system of intercourse or colonization by the exclusory regime of our British

Rajahs. Hence we at once perceive a reason why, on the return of peace in 1763, the spirit of national curiosity was prevented from taking that bias which might have led to a wish for more complete information regarding this rich and interesting part of the world.

On the other hand, the minds of the learned and the speculative had been long agitated by the subject of a Southern Polar Continent. During the 17th century, many discoveries made in the Pacific by the distinguished navigators Tasman, Quiros, Dampier, &c. but detailed in such a manner as threw over them an air of mystery and romance that tended to whet public appetite, gave the impulse in that direction to a feeling which indeed had no other outlet whereby to vent itself. Accordingly a succession of expeditions on the laudable and benevolent principle of enlarging the boundaries of natural knowledge, was persevered in for a period of nearly twenty years, under officers of the highest reputation, one especially, the most accomplished navigator the world has ever beheld. These expeditions, and subsequently other projects, such as the transplanting of the bread-fruit tree from the Society to the West India Islands—objects connected with Nootka Sound, the survey of the north-west coast of America, the pursuit of the fur trade, occupied the notice of Government, and kept the national attention awake till the war of the French revolution in a great measure put an end to all peaceful arts. The importance of New South Wales as a convict colony rendered a hydrographical examination of its coasts necessary. This led, during the short armistice of 1801, to the sending out of Capt. Flinders, whose premature and lamented death furnishes an instance of tyranny the most cruel and refined of which modern history has given any example, and stamps indelible infamy on the Government that sanctioned it. Europe has rung with indignation at the violence done to all the principles and usages of civilized life, and even of humanity, by the treatment which this most able and zealous officer underwent at the Isle of France. It was not the hardship arising from a few months' detention, but a lingering captivity of seven long years, under vexations destructive of his health, and ultimately of his valuable life. The great things which, with even small and limited means, he was able to effect may be regarded as a supplement to that course of nautical investigation of which I have spoken. The distractions of a fresh war enforced a pause in all the more speculative pursuits of Government, nor was it till the year after hostilities had entirely ceased that sufficient leisure could be obtained for meditating any exploit worthy of our character. At that period an expedition connected with operations that had been some time in progress for penetrating into the interior of Africa, was sent to the river Zaire on the south-west coast of that continent. The unfortu-

nate result of that expedition is too well known. Shortly after, the tide of public attention set towards the North Pole under the fallacious hope of there finding an open basin of water. This current, which a single summer's excursion to Spitzbergen and round the head of Baffin's Bay served all of a sudden to freeze for ever in a manner not yet well accounted for, immediately took a more westerly direction, and the minds of all people became filled with a desire of solving the grand problem of a North-west Passage. This object, so greatly interesting to geography, has since continued to engross almost entirely the public anxiety. To encompass it, a series of sea and land expeditions, in their nature imminently perilous, have been performed with an ardour and perseverance not more creditable to the projectors than to the devotedness and enduring fortitude of those to whom the execution was entrusted.

While these are pending, hardly any thing of sufficient moment occurs to engage public curiosity; and as from their sameness and repetition the edge of novelty and the pain of anxiety are much worn off, they have, in a great degree, ceased to produce that intense interest which they at one time excited. Other fields of enterprize and inquiry must, therefore, be sought for, and one in every way productive opens on our view. Over the numerous islands of the Indian Archipelago nature has poured her bounties and her beauties with an unsparing hand. These gems of the ocean are the native soil of almost all those rare and precious herbs and fruits which minister to man's enjoyments when in health; to his relief when under disease. Yet more is known of the fungi and the lichens growing on the barren rocks of Melville Island, than of the fragrant health-giving plants of the Moluccas! As articles of commerce we see them, it is true, in the packages of the merchant's warehouse—from certain unauthentic sources we gather that they flourish here or there, but of their minute botanical history we hardly know any thing precise.

A few species of insects and fishes from the more frequented commercial depots are sometimes brought over to furnish food for speculation, or to raise an eager but fruitless desire for further information on the part of naturalists; while the exhaustless swarms of entomonic and ichthyonic life that every where abound in those regions are to us as though they were not.

We have "travelling the country" that *rara avis* the black swan, and other birds from New South Wales, Van Diemen's Land, &c. while the myriads of the feathered tribes that haunt the Archipelagan Seas and Islands are left to be guessed at from a few dried skins occasionally finding their way into our cabinets.

The history of the kanguroo, ornithorynchus paradoxus, &c. is familiar as household words in the mouths even of our chil-

dren, while of the multitudes of quadrupeds that inhabit the Indian Isles, we know very little, except when, now and then, a specimen chances to meet our eye in the travelling showman's caravan, brought home by some captain of the sea, and appended to it some idle tale fit only for the nursery.

Nay, even of man as he exhibits himself in those climates, we have notices the most meagre and marvellous, though his history, in regard to many essential points, be there both curious and important, while in the remotest islets of the South Sea, every variety of colour, feature, and form, under which he appears has been delineated with the utmost degree of minuteness.

Though I am ready to admit that the general view here taken is open to some few exceptions, yet I know that in the main it is correct. Had all that has been just urged taken place with respect to China or Japan, those countries of civilized barbarians, who have shut themselves up from the rest of the world by the impassable barriers of despotism and prejudice, we might have felt disappointment and regret. But happening in regard to countries that hold out every enticement to our research, every creek of which is approachable, on seas over which our powers of locomotion are unlimited, it does assuredly seem strange that so few attempts have been made to gain an acquaintance from which advantages immediate, important, and permanent, must have resulted.

I have said that my statement is liable to a few exceptions, and they are few; for I really am not aware of the existence of any works to which the stamp of truth can be affixed, beyond those of Marsden, Raffles, Crawford, Dr. Jack, and of Dr. Horsfield. (Perhaps to this list, a small number of foreign works, but of inferior reputation, may be added.) These, however, have all, except Marsden's Sumatra, made their appearance since the period when the comprehensive and energetic mind of Lord Minto furthering the liberal views of the British Government at home, burst the spell of directorial thraldom and Dutch tyranny at the same moment, and brought a little into play the activity and genius of the country. During those fortunate years when Java and its dependencies were under the British dominion, and which may well be styled their years of jubilee,* much was effected, and of the vast harvest still remaining to be reaped, particularly in the departments of natural history, some idea may be formed from the earnest given by the distinguished authors and naturalists now cited. All, however, that has been effected, is but partial and incomplete, as indeed I may fairly infer from

* Unhappily for the cause of science and humanity, the bright prospects which the possession of Java by Britain held out, have been blasted by the restoration of that highly important island to its Batavian masters, whose baneful policy will probably soon undo the whole of that beneficent system instituted by our Government for ameliorating the condition of an intelligent, attached, and rapidly improvable people.

the candid acknowledgements of the writers themselves, as well as from the avowed purposes and pretensions of their works.

It remains then for a wise and generous government to follow up by its influence and its power the advantages already gained for science and natural knowledge, by extending the limits of survey to those interesting portions of the earth. The present is a singularly propitious season. The blessings of peace and plenty have been for some time felt and enjoyed. Government has leisure, and it has manifested the disposition to cultivate commercial relation on the broad and truly beneficial basis of reciprocity. Moreover, independently of the considerations due to science and to the interests of mankind, there are not wanting others that touch this country more nearly than some are perhaps aware of.

The Charter of the India Company must in a few years expire, and the management of that unwieldy machine * lapse into the hands of Government. The prohibitive system will then, in all probability, give way to one more consonant to the free spirit of commerce and sound policy. Should this happen, it would seem greatly desirable that the trade which would then be opened to British capital and enterprize, should commence under circumstances the most favourable, and that our merchantmen should not have to navigate unknown seas, but should be provided with maps, and charts, and soundings, from the (now) only authentic source, *our own Admiralty*, that they may be as little as possible dependent on the clemency or caprice of semi-barbarians.

But if, contrary to every reasonable expectation, the treasures of *commerce* must still be withheld from our grasp by the renewed ascendancy of the Company, let not the treasures of *knowledge* be hid from us. This at least is within the controul of Government, nor can the ascertaining the actual condition of the Archipelagan Islands be deemed any infringement of the Company's rights.

An expedition with this object in view would not partake so much of *discovery* as of *survey*. The islands to be visited are all known to exist; no time need be lost in endeavours to find out *new* lands, but every hour would afford scope for industry and skill in identifying and tracing the *old*; in inquiring into the customs, languages, character, and physical peculiarities of the inhabitants, the climate, soil, productions, capabilities, &c. of the islands; the most eligible points of colonization or commercial establishment, and all the other multifarious objects which the wisdom of Government would not fail to embrace. From a

* Those who may be inclined to think that I have spoken with but little ceremony of the East India Company, I would refer to the works of Staborinus, Von Hogendorp, Sir Stamford Raffles, and Mr. Crawford, in which facts and opinions are stated that will doubtless increase in importance, as the period draws nigh when the affairs of this Corporation must be subjected to parliamentary revision.

three or five years' expedition of this kind, or from several combined expeditions, conducted by intelligent officers, aided by the experience of Sir S. Raffles, Dr. Horsfield, Mr. Crawford, and other eminent naturalists and oriental scholars, what brilliant acquisitions might we not anticipate! Comparatively small risk —abundant means, and leisure to collect and mature observations—constant supplies of fresh provisions—within reach of our own or friendly settlements, whence as often as might be accounts could be regularly transmitted home, and from time to time published; in short, such a field presents itself as for extent and productiveness exceeds all calculation.

I may add besides, what to many is becoming a source of surprise and disappointment, that no exploit of any magnitude seems in contemplation to adorn the annals of the present reign. It will be remembered that all the magnanimous and disinterested expeditions sent out by Britain, originated during the reign of our late venerable monarch, and that however important the enterprizes that have been brought to a conclusion since the accession of George IV. they are but the sequel of that system of generous enterprize which began, but which, I trust, did not end with the reign of George III. And it will likewise be kept in mind that though the course or termination of the Niger should be found out, or the North-west Passage effected,* or the non-existence of the latter determined, the merit and the distinction, whatever these may be, will, according to the strict letter of chronology, attach to the memory of that Prince under whose *reign* the expeditions were first planned, and partly performed. The consequence will be, that little of this *kind* of glory, whatever there may be of any other, will be left to form a wreath for the brow of our present gracious Sovereign.†

Nor should it be wholly forgotten that the French and American governments may, if we should not, avail themselves of the present interval of universal repose, to equip expeditions‡ that might forestall many of the benefits which are ours by prescriptive right, and which are now ready, if we be not found wanting, to drop into the lap of our national prosperity. Some may be

* We cannot stifle our regret at having seen intimated, though in an unofficial shape, that all further nautical attempts at a North-west Passage are to be laid aside. Our sole reliance then must now be on Capt. Franklin, for I should witness with the deepest regret the noble object which had so long occupied the energies of a Parry and his brave associates, snatched from our gripe by the serfs of a Russian Boyar.

† We are not unmindful that all the late arctic expeditions, with the one to the Coast of Guinea commanded by Capt. Tuckey, were sent out under the auspices of his present Majesty when Prince Regent.

‡ This prediction has been in part fulfilled since these remarks were put to paper, and is one of the principal reasons why they have been obtruded in their present imperfect shape. A French discovery ship has been actually set apart for the coast of New Guinea, and the President of the United States has, in a late Message to Congress, revived a design of Washington's, to survey the entire coast of America. These expeditions, though limited to special purposes now, may be extended, or may *beget others.*

ready to cry out here, "So that the benefits become ours, no matter through what channel they reach us." This is not our sentiment, for so inveterately are we imbued with national partiality that we should wish our own Britain to engross, if possible, every great atchievement to herself. I object also on other grounds to having any participators in the scheme which I have now been so earnestly drawing. The conduct of France in all that relates to the voyage of the navigators Baudin and Hamelin, and their transactions with Flinders, coupled with what we know of the influence of public opinion in that country, show sufficiently what the world has to expect from a French expedition.

Again, though in America the freedom of the press be recognized to its utmost extent, I distrust the ability of those who may be employed to perform the work in that finished style which the advanced state of knowledge renders indispensable. With Britain it is otherwise. Her openness of proceeding, the vastness of her reputation, her proved ability, point her out as the only power capable of executing the task satisfactorily. She has now reached that pre-eminence which entitles her to dictate, and ensures the utmost attainable perfection to whatever undertaking she applies herself. She has hitherto taken the lead in all that is truly great and liberal in nautical affairs. Her position being withal insular, the character and habits of her people are of necessity maritime and commercial, and their genius bold and enterprizing. No effort, therefore, should be neglected that can in any way tend to exalt those peculiarities by which they are honourably distinguished from any people now existing, or that ever did exist, and enable them to maintain their high-reaching "pride of place" among the nations.

In one word, the thing is to be done, and *Britain must do it*.

H.

Article VIII.

Some further Remarks on the Specific Gravity of Hydrogen, and on Dr. Prout's Modification of the Atomic Theory, in Reply to Dr. Thomson. By Harry Rainy, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Glasgow, Feb.* 15, 1826.

In a former paper I made some remarks on the new experiments on the specific gravities of oxygen and hydrogen, recently published by Dr. Thomson in his valuable treatise on the Principles of Chemical Philosophy. These experiments were considered by Dr. Thomson, as distinctly proving that the specific gravity of oxygen is to that of hydrogen exactly as 16 to 1, thus affording a strong confirmation of the truth of Dr. Prout's hypo-

thesis. I endeavoured to show that when a proper correction was made for the influence of vapour, the experiments indicated that the specific gravity of oxygen is to that of hydrogen as 16·54 to 1, and therefore are opposed to Dr. Prout's views. In an answer to my paper (New Series, vol. x. p. 352), Dr. Thomson states that the dilute acid which he employed for disengaging his hydrogen consisted of 1700 grains of water to 400 grains of sulphuric acid, that its boiling point was 224°, and, therefore, that hydrogen evolved from it at 49° would contain vapour of a tension not exceeding that from pure water at 37°, which by Dalton's table is 0·237. If the correction for vapour is made on these data, Dr. Thomson shows that it will follow from his experiments that the specific gravity of oxygen is to that of hydrogen as 16 to 1·0077. In the detail of the experiment given in the Principles (vol. i. p. 70), after mentioning that the experiment was performed at temperature 49°, Dr. Thomson states expressly that "the specific gravity of vapour at 49° is 0·00533." I certainly did not suppose that by vapour of 49°, Dr. Thomson could mean vapour of temperature 49°, but of a tension corresponding to 37°, especially as 0·00533 is pretty nearly the specific gravity of vapour at 49°, if calculated by a rule which Dr. T. himself has given in the *Annals* (New Series, vol. iii. p. 305). I did think it likely that the tension of vapour from the dilute acid would be somewhat less than from water; but as Dr. Thomson had not stated the strength of his acid, I had no means of estimating the difference, and from his own words above quoted, I supposed that he considered the difference as too minute to require particular notice.

I believe considerable doubts are now entertained respecting the correctness of the method of estimating the tension of vapour from a liquid by comparing its boiling point with that of another liquid, of which the tension at the given temperature is previously known. This is the method adopted by Dr. Thomson. I thought it would be satisfactory to take the tension of the dilute acid directly. With this view I prepared a mixture, containing by weight 1700 parts water to 400 of the strongest liquid sulphuric acid. This mixture had the specific gravity 1·157, and in various careful trials, *I found its tension at* 49° *to be* 0·32, *instead of* 0·237 as calculated by Dr. Thomson. In order to find its boiling point, I heated it in a platina crucible over a lamp. It boiled briskly at 217°. I kept it boiling till its temperature rose to 224°. Being covered up and cooled, 185·24 grains of it dissolved 39 grains of zinc; whence if we admit with Dr. Thomson that the atoms of zinc, sulphuric acid, and water, are respectively as the numbers 4·25, 5 and 1·125, it will follow that the acid which boils at 224° consists of 400 anhydrous sulphuric acid to 1210 water, or of 400 strongest liquid acid to 918 water. Dr. Thomson has not stated whether his acid consisted of 1700

parts water to 400 of the strongest liquid acid, or to 400 anhydrous acid; but even on the latter supposition, if my experiment is correct, its boiling point must have been considerably below 224°; and as the experiment was twice repeated without any material variation, I do not see any reason to doubt its accuracy. If Dr. Thomson took the boiling point of his acid in a glass vessel, it would account in some degree, though by no means entirely, for the difference in our results. I think it unnecessary, however, to make any further remarks on the experiment detailed in the Principles, as Dr. Thomson himself seems now not entirely satisfied with it; and as he states that out of ten similar experiments, this was the only one that appeared to yield satisfactory results, I am somewhat surprised that he should ever have placed much confidence in it, and especially that he should have adopted it as a basis on which to raise such an important superstructure.

In the new experiment which Dr. Thomson details in his last paper, he endeavours to deprive the hydrogen entirely of moisture, by passing it through 37 inches of tube filled with chloride of calcium, which he had found was sufficient to render the hydrogen gas as dry as it could be made by this method. There are several objections to our admitting this experiment as sufficiently delicate to enable us to decide on the truth of Dr. Prout's hypothesis.

1. The great weight of the flask, and tubes and their contents, compared with that of the hydrogen to be weighed. The apparatus with which the beam was loaded weighed more than *a thousand times* as much as the whole of the hydrogen; it is not to be expected, therefore, that the absolute weight of the hydrogen could be ascertained with minute precision. This has been distinctly adverted to by Dr. Thomson himself.

2. The manner in which Dr. Thomson removes the hydrogen in the tubes, and replaces it with common air, is liable to objection. When Dr. Thomson inhales air saturated with moisture at about 60° through 37 inches of tube filled with the chloride of calcium, some of the moisture must be retained, and must add to the weight of the tube, and consequently lead the experimenter to underrate the weight of the hydrogen which has escaped. In order to obviate this source of error, the air inhaled should have been previously dried by chloride of calcium.

3. It results from this experiment, even supposing it quite correct, that the specific gravity of hydrogen is to that of oxygen as 1·0077 to 16, or in other terms as 1 to 15·87. This certainly approximates pretty nearly to the ratio of 1 to 16; but still the difference is by no means immaterial. The deviation from an integer is 0·12, or about one-eighth, which seems small, but we must remember that the *utmost possible deviation* is 0·5, for were

the experimental result to vary from 16 by more than 0·5, it must approach some other integer. The variation from the theoretic result in this experiment is actually about one-fourth of the greatest possible variation. Dr. Thomson would probably reply, that the greatest possible variation would be 1 and not 0·5, if we compare the atomic weights of oxygen and hydrogen, instead of their specific gravities; but even admitting this, the deviation from the theory is still about one-eighth of the greatest possible deviation. *All numbers which have large ratios to any given number must of course nearly coincide with integral multiples of that number*, and the coincidence must be so much the nearer, as the ratios which the former have to the latter are larger. This is precisely the case with hydrogen. Its atomic weight compared with other bodies is extremely small, and, therefore, if we assume it as unity, the atomic weights of other bodies must nearly be integers. Were astronomers to adopt the mass of the moon as unity, they might represent the masses of the primary planets by integers, with great precision, yet I suppose no one has ever fancied that the latter were exact integral multiples of the former. Dr. Thomson will himself admit that the truth of the hypothesis could scarcely be ascertained, were the proofs to rest on a direct comparison of the atomic weights of mercury, iodine, or even chlorine, with that of hydrogen, because even the lightest of these atoms, that of chlorine, is about 36 times as heavy as the atom of hydrogen. It would not be at all surprizing, from the imperfections to which even the most refined chemical researches are still liable, that the actual number should differ by more than even a whole unit from the above number. Davy's experiments led him to adopt a number which on this scale would be $33\frac{1}{2}$. I believe Dr. Thomson's number to be nearer the truth; but how are we to decide whether the true atomic weight is an *integer*, when two sets of experiments performed with great care indicate a difference of *two units and a half* in the result. To ascertain the truth of the theory, we must have recourse to atoms of which the weights do not bear such a large proportion to that of hydrogen. Such are the atoms of carbon, oxygen, and azote. Could it be proved clearly that there are integers on the hydrogen scale, there would be strong reasons for believing that the other atoms are also integers. This might at first appear a simple task; but we must remember that the object in view is not to discover the integer which most nearly coincides with the true number; but to discover whether or not the true number be precisely an integer, and not a fraction. The method of ascertaining the relative atomic weights from the relative specific gravities of the gases, appears to afford by far the most accurate results; and yet from the impurity of the gases, and from the small quantities that can be weighed, it is extremely difficult, if not impossible in the present state of the

inquiry, to obtain sufficient data, either for refuting Dr. Prout's theory, or establishing it on a solid basis. I do not by any means object to Dr. Thomson's numbers as very good approximations, probably the best which we possess at present; but I am as yet unable to see any satisfactory proof of their absolute exactness. Dr. Thomson observes that in Great Britain at least, the specific gravities which he assigns for oxygen and hydrogen are universally admitted to be true, but I am persuaded that they are admitted by no means in the rigid mathematical sense in which he holds them.

Dr. Thomson, in the conclusion of his paper, has stated some other proofs, which appear to him perfectly conclusive in favour of Dr. Prout's hypothesis. I shall make a few remarks on each in the order in which they are stated.

1. Dr. Thomson "determined by actual experiment, that the specific gravity of hydrogen gas is 0·0694; and the subsequent determination of Berzelius and Dulong approaches very nearly to this number." *Now Dr. Thomson's experiments were made on moist gas at a temperature which he has not stated.* We cannot, therefore, ascertain from them the precise specific gravity of hydrogen; they merely prove, if correct, *that the specific gravity of dry hydrogen is less than* 0·0694. If the experiments were made at 60°, it would follow from them that the specific gravity of hydrogen is about 0·062, and consequently that the specific gravity of oxygen is 18 times as great as that of hydrogen, and that the atom of oxygen is 9 times as great as that of hydrogen. Even were the experiments made at 32°, the results would be totally inconsistent with Dr. Prout's hypothesis. It was in fact these very experiments that first led me to question the truth of Dr. Prout's hypothesis. When these experiments were performed, Dr. Thomson did not admit that moisture has any material influence in modifying the specific gravity of the gases (see *Annals*, New Series, vol. iii. p. 302—308); but now that he admits that influence, I conceive he must also admit that his experiments are hostile to Dr. Prout's views.

2. Dr. Thomson thinks he has adduced conclusive evidence in his Principles that atmospheric air consists of 4 volumes azote to 1 volume oxygen. I am by no means satisfied with this evidence. The results were not uniform. In one instance, Dr. Thomson found 79·246 azote, and in another 80·927, and though the mean of 10 experiments, 79·97, approaches extremely near the theoretic number, it differs from previous experiments made by Dr. Thomson himself, as well as by other experimenters. But even were we to admit this point, I do not see how it follows that the specific gravity of oxygen is 1·1111, unless we previously know either the specific gravity of azote, or the ratio of the atomic weights of oxygen and azote. Dr. Prout extricates

himself out of this difficulty by *assuming* that the atom of azote is to that of oxygen as 1·75 to 1, and then infers that the specific gravity of azote is 0·97$\dot{2}$.

3. "The specific gravity of ammoniacal gas deduced from a mean of Sir H. Davy's experiments and Dr. Thomson's is 0·590237. It has been satisfactorily proved that it consists of 1 volume azote and 3 volumes hydrogen condensed into 2 volumes. Hence if we admit that the specific gravity of azote is 0·97$\dot{2}$, it will follow that the specific gravity of hydrogen is 0·069417." This evidence appears at first sight satisfactory; but if we assume Dr. Thomson's experiments on ammoniacal gas, as the basis of calculation, we shall find the specific gravity of hydrogen to be 0·071, or 0·073 if we take 0·59669, the specific gravity of ammonia assigned by Biot and Arago. I see no reason for giving a preference to Sir H. Davy's result in this case, except that it coincides more closely with the hypothesis.

4. Adopting the specific gravities of oxygen and hydrogen, as determined in the last two paragraphs, it will follow that the atom of hydrogen is to that of oxygen as 1 to 8·003, a coincidence so near that we may safely assume the real ratio to be 1 to 8. This of course I admit, but the accuracy of the conclusion rests entirely on the correctness of the premises, which I have already called in question.

5. "The specific gravity of vapour upon which Mr. Rainy lays so much stress, is founded on the assumption of this ratio. The specific gravity of vapour has been settled at 0·625. Now vapour is a compound of 1 volume of hydrogen and $\frac{1}{2}$ volume of oxygen united together, and condensed into 1 volume. If we subtract 0·55$\dot{5}$ from 0·625, the remainder 0·069$\dot{4}$ must represent the specific gravity of hydrogen gas, and 0·069$\dot{4}$: 1·1111 :: 1 : 16. Therefore all the calculations and objections of Mr. Rainy were founded on the admission of the very ratio which he endeavours in his paper to overturn."

I have no doubt that the specific gravity of vapour is nearly 0·625 or 0·624, as ascertained by Gay-Lussac. I did not assume 0·625 as mathematically correct, but partly because I did not wish to differ with Dr. Thomson on a point which had no material bearing on the argument. It was obvious to me that the number 0·625 was not affected with any error that could at all interfere with my conclusions. Supposing the specific gravity of vapour to be as low as 0·6, or as high as 0·65, the objections which I have urged still seem to me unanswered. But admitting that the specific gravity of vapour is rigidly 0·625, and that each volume of it contains 1 volume of hydrogen and half a volume of oxygen, I deny that from these data any inference can be made regarding the ratios of the specific gravities of oxygen and

hydrogen. Let us call the specific gravity of hydrogen y, and that of oxygen x, it follows from the above statement that

$$y + \frac{x}{2} = 0{\cdot}625.$$

This is an indeterminate equation, which of course discloses nothing with regard to the *ratio of* y *to* x. They admit of an infinity of values. If we put $y = 0{\cdot}073$, we shall find $x = 1{\cdot}104$, which were nearly the numbers pitched on by Gay-Lussac. We shall find $x = 1{\cdot}11\dot{1}\dot{1}$ if we make $y = 0{\cdot}069\dot{4}$, but that is one of the very points in dispute. Therefore there is no inconsistency in supposing that the specific gravity of vapour is 0·625, and yet that Dr. Prout's hypothesis is unfounded.

Dr. Prout proposed his views, as a probable conjecture, not inconsistent with the established facts of chemistry, and, therefore, deserving of further inquiry. Dr. Thomson adopted them as completely proved, and seems to think that they must be admitted to be true, if they cannot be proved to be false. He accordingly introduced them into his System, and did not scruple in any instance to modify experimental results so as to correspond with them. Dr. Thomson's System has long been considered a standard work in chemistry, and probably has contributed more to diffuse correct views of chemical science than any treatise that ever was published in Britain. I think it is deeply to be regretted, that in a work in every other respect so valuable, most of the experimental results should be vitiated, by modifying them to suit an hypothesis, which, whether it be true or false, will tend to retard the science, if adopted prematurely and on insufficient evidence.

Dr. Thomson has well remarked that this doctrine gives a mathematical precision to chemical research, which the most sanguine cultivators of chemical science could scarcely have anticipated. But just in proportion to the importance of these views, is it necessary rigidly to examine the evidences of their truth. The evidences hitherto adduced appear to me, for the reasons already stated, to be by no means satisfactory. Some of them are even inconsistent with the truth of the hypothesis. I would not, however, be understood as asserting that Dr. Prout's views are incorrect. I think it is still a matter open for inquiry; and it is a very remarkable circumstance, that though more than ten years have now elapsed since Dr. Prout's paper was published in the *Annals*, no chemist, except Dr. Thomson, should have engaged in any experimental researches on a subject so highly important.

While I thus freely state my objections to the reasonings of Dr. Thomson, I hope I have done so with that deference which is due to his talents and experience. The subject evidently

requires further elucidation, and I am sure Dr. Thomson is too candid to object to its being fully discussed, however clear the evidences may appear to his own mind.

HARRY RAINY.

ARTICLE IX.

Descriptions of Two new Minerals. By Mr. A. Levy, MA. FGS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

AMONG many very interesting specimens, and several undescribed substances, which were in the collection of Marquis de Drée, and which Mr. Heuland, who lately bought it, has added to his own private collection, are the two of which I now send the descriptions for insertion in the *Annals of Philosophy*, and which belong, I believe, to two new species. For one of them I propose the name of Beudantite, in honour of Mr. Beudant, and for the other, at the suggestion of Mr. Heuland, the name of Königine, in compliment to Mr. König, of the British Museum.

Königine.

The characters which distinguish this substance from any hitherto described, were ascertained before the arrival of Marquis de Drée's collection into this country upon a specimen now in the possession of the Dowager Countess of Aylesford. The specimen which subsequently was found to belong to the same species had the following ticket (*cuivre muriaté et phosphaté*). In both cases the mineral occurs in small crystals, emerald-green and blackish-green, translucent, and of either of the forms represented by figs. 1, 2, 3. These crystals cleave only and very easily in a direction parallel to the plane P of the drawing; the face obtained by cleavage is very brilliant, and at right angles to the lateral planes *m*. These lateral planes are generally dull

Fig. 1.

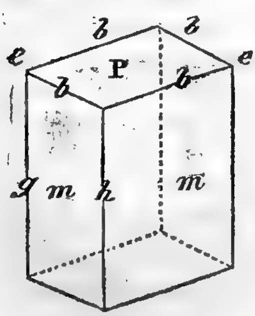


Fig. 2.

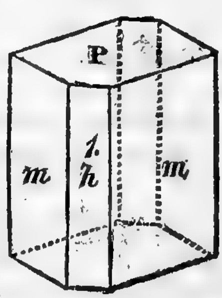
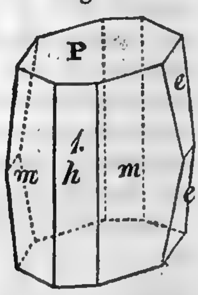


Fig. 3.

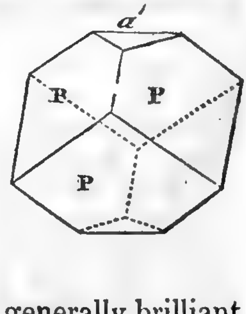


and slightly curved, and this last circumstance gives to the crystals, which are rather elongated in the direction of the axis, something of a barrel shape. For the same reason the incidence of the lateral planes could only be obtained by approximation by means of the common goniometer, and appears to be about 105°. A right rhombic prism of 105° may, therefore, be considered as the primitive form. The ratio between one side of the base and the height has not been determined, because the angle of P on *e*, fig. 3, could not be measured with sufficient accuracy. The hardness of the substance is nearly the same as that of sulphate of lime, and it is very easily pounded. The crystals are closely engaged together, and are placed upon amorphous ferruginous oxide of copper. The locality is Werchoturi, in Siberia. From the examination of a very small quantity of this substance by Dr. Wollaston, it appears to consist principally of sulphuric acid and oxide of copper, and might, perhaps, be considered as a subsulphate of copper. This result shows a great analogy between Königine and Brochantite, which last mineral, examined by Mr. Children, was also found to consist principally of sulphuric acid and oxide of copper. The hardness, colour, matrix, and locality, of the two substances seem also to be nearly the same, but their forms appear to be totally different. Brochantite occurs in thin rectangular tables whose angles are truncated and edges bevelled, without any appearance of cleavage. Königine, on the contrary, is met with in barrel-shaped crystals, with an easy and most brilliant cleavage in a direction perpendicular to the axis. When these two very rare substances may be procured in large quantities, it will, however, be worth the attention of mineralogists and chemists to compare them again.

Beudantite.

This substance occurs in small crystals closely aggregated, of the form represented by fig. 4, which is a slightly obtuse rhombohedron with the summits truncated. Their colour is black at the surface, and their lustre somewhat resinous, but thin fragments are translucent, and of a deep-brown colour. They cleave easily in only one direction parallel to the face *a′*, or perpendicular to the axis of the rhombohedron. This face of cleavage, however, is not sufficiently brilliant to allow the use of the reflective goniometer to measure its incidences upon the planes of the rhomboid. These planes themselves are generally brilliant, but sometimes slightly curved. The mean of several measurements obtained by the reflective goniometer has given for the incidence of P on P, 92° 30′. The primitive form of Beudantite

Fig. 4.



is, therefore, an obtuse rhomboid of 92° 30′. The hardness is sensibly greater than that of fluate of lime. When pounded, the colour is of a greenish-grey. The matrix seems to be the same substance in an amorphous state with veins of fibrous hematite; it comes from Hohrhausen, on the Rhine.

I am also indebted to Dr. Wollaston for the chemical examination of this mineral, the result of which is very interesting, the only substances he has been able to detect in it being oxide of lead and oxide of iron.

ARTICLE X.

Description of the Process of Amalgamation as carried on in Germany. Extracted from a Letter to John Taylor, Esq. from John Henry Vivian, Esq.

AFTER some prefatory observations, Mr. Vivian proceeds to describe the Freyberg processes, which are stated to be thus conducted.

The silver is extracted from the ores of the mining districts of Saxony, partly by amalgamation, and partly by smelting; or, to make use of technical terms, some ores are treated in the *wet*, and some in the *dry* way. There are two works for smelting, and one for amalgamation, in the neighbourhood of Freyberg, which is the capital of the mining districts of Saxony. I shall confine myself, in the present paper, to the consideration of the processes carried on in the latter.

The Amalgamation-work is situated in a valley, near the village of Halsbrücke, at a distance of about two miles from the town of Freyberg, on the small river Mulda. I may here observe, that in selecting a site for a work of this description, regard must be had not merely to the proximity of the mines from which the ore is to be obtained, but to the supply of water, which should be constant, and in sufficient quantity for all purposes for which it may be required. With respect to the arrangement of the interior, the work at Freyberg may safely be taken as a model, or at all events a description of it will afford many useful hints in forming a similar establishment. Every thing is done with a view to save manual labour, and to prevent a loss of ore or metal in their removal from one place to the other, or pilfering on the part of the men. At the close of my remarks, after describing the processes, I shall revert to this part of the subject, which will be then the more readily understood.

The processes in an amalgamation-work are grounded on chemical principles. From this circumstance, and from the order and method that pervaded every part of the Freyberg esta-

blishment, the extreme neatness and cleanliness with which it was kept, and the skill and science with which the processes were conducted, together with the civility and attention of the agents and workmen, I took a peculiar interest in the establishment, and frequently visited it during my residence in the neighbourhood.

It may be well to give a general outline of the processes, before I enter on the details, that the connecting links of the system may be understood.

The first operation that requires description is the selection of ores to form a proper mixture, with reference to the quantity of silver and sulphur they contain. This is a most material point to be attended to. It has been before observed that the amalgamation process succeeds best, when the silver produce is about $7\frac{1}{2}$ *loth* in the quintal of ore, or about 75 ounces in the ton. The object, therefore, is, by a selection of different ores, to bring the whole as near to this average as can be conveniently effected; at the same time regard being had to the proportion of sulphur contained in them. This is estimated by the quantity of regule, or sulphuret, found in the ore; which is ascertained by an assay in the crucible. The standard by which they are governed is, that a proper mixture of the different ores gives 35 per cent. of regule; about one-half of which, or perhaps rather more, may be sulphur. But as the silver in the Freyberg ores is rarely in the metallic state, at least in any quantity, it becomes necessary to detach it from its combination with sulphur or other substances, before subjecting it to the actual process of amalgamation; as otherwise these substances would prevent its union with the mercury. This is done by adding to the mixture of raw ore, 10 per cent. of common salt, or muriate of soda; by which, during the operation in the furnace that follows, a chemical change is effected. The sulphur becomes acidified, and the acid thus formed, uniting with the base of the salt, forms sulphate of soda; whilst the muriatic acid thus set free, combines with the silver in the ore that was not in the metallic state, and forms muriate of silver.

In this state the ore is subjected to various mechanical operations, with riddles, screens, mills, and sieves, until it is reduced to an impalpable powder. It is then submitted to the action of the mercury, which is the actual process of amalgamation. This is performed in barrels, which are so arranged as to revolve on their axis. The mixture or charge in each barrel consists of sifted calcined ore, mercury, metallic iron, and water, in certain proportions. The ore is composed of sulphate of soda, muriate of silver, muriate of iron, and other metals and earthy matters. By the process of amalgamation, the barrels being made to revolve during a period of 16 or 18 hours, the muriate of silver becomes decomposed by the action of the iron on its acid, and

the silver thus reduced to the metallic state, combines with the mercury, forming what is termed amalgam; whilst the sulphate of soda, the muriate of iron, and other salts, become dissolved in the water. The silver combined with mercury is then filtered, by which the surplus metal is separated, and a compound remains in the sack, consisting of six parts of mercury and one of silver. This amalgam is subjected to the action of heat in a distilling furnace, by which the mercury is sublimated, and the silver remains. The silver is then collected, and melted in a crucible; but as it contains a portion of other metals that were combined with it in the ore, it is refined in a cupel or testing furnace. The residue from the barrels is washed in large vessels, and the particles of quicksilver still remaining, are carefully collected.

Such is the general outline of the processes: I shall now consider them in detail.

The ores selected for amalgamation at Freyberg are of two descriptions; such as contain little or no lead or copper, but yield from 5 to 8 *loth* of silver in the *centner* or hundred weight; and stamped ores, that are dressed, clean, and free from earthy particles. Each parcel of ore, when received at the work, is deposited in a separate hutch, and its contents in silver and regule, with the particulars of its component parts, and the mode in which it has been prepared, are carefully noted in an ore book. By this arrangement, the requisite mixture is formed with the greatest accuracy. The quantity of ore thus prepared at one time is 400 *centner*, which is a week's work for four calcining furnaces. The contents of this lot in silver, should not exceed 200 marks. The parcels of ore selected for the general mixture are wheeled from the ore-house, and spread with great nicety over the floor of a spacious apartment, situated immediately over the calcining furnaces. The ore is deposited in layers, of from three to four inches in thickness; the dry ores are regularly intermixed with those from the stamping houses, and a layer of salt is placed over one of ore. The salt, which has been screened to prevent any lumps from remaining, is let down on the ore, by a pipe which communicates with a wooden case on the floor above, formed so as to contain the requisite quantity. A heap from $2\frac{1}{2}$ to 3 feet in depth is thus formed of alternate layers of ore and salt. As each layer is carefully spread over the surface of that beneath it, a regular mixture of the whole is obtained, on being cut vertically through. When the whole has been well mixed, it is separated into portions or charges, of 4 cwt. or two barrow-loads each. These are removed, as it is found necessary, to a part of the room, the floor of which is covered with tiles, and is a little lower than where it was mixed. These charges are laid in separate heaps, ready for the furnaces, which are on the ground-floor, immediately under; so

that each charge can be let down through an opening communicating with the interior of a furnace. The calcining furnaces are reverberatory, and of small dimensions.

Four of these calcining furnaces are arranged together; that is, in two rows, with a common stack in the centre. They are surrounded by a screen, which prevents any vapours that may escape through the front door from finding their way into the building. The space within the screen communicates with the flue.

The form and arrangement of a furnace for this purpose may be modified according to circumstances and the nature of the fuel to be employed. The charge of ore in the Freyberg furnaces will appear to those accustomed to the large reverberatory furnaces of this country, which often contain from 60 to 70 cwt. of ore, as exceedingly small. It is possible that the Saxon furnace may admit of being enlarged with advantage, or perhaps improved in form; but it should be borne in mind, that the operation to be performed in it, is not a simple calcination of ore, preparatory to its being brought into fusion, but a process in which certain chemical changes and combinations are to be effected, on which the success of the whole amalgamation system depends; and, therefore, to overcharge the furnace, or to make it of such dimensions as might not be in every respect most convenient and manageable, with a view to a trifling economy of fuel, would be highly improper.

On the part of the workmen, the utmost attention is required to prevent the ore from caking together, which, from the moisture contained in the salt, it is disposed to do; especially it is necessary to avoid the slightest degree of vitrification. The charge should be kept well rabbled, and should be occasionally shifted from one part of the furnace to the other, that the whole may be equally exposed. The heat should also be regulated according to the state of the ore. In the beginning, it should be very moderate, and should be gradually increased as the process advances. At Freyberg, when the ore becomes thoroughly heated, the sulphur begins to burn; and when this is perceived to be the case, the fire in the grate is slackened; and instead of wood, which is the fuel employed in the first part of the process, a small quantity of coal is thrown on, just to keep the fire alive. The burning of the sulphur generally continues from two to three hours; and after it has ceased, the fire is increased, and a tolerably strong heat is kept up for about another hour, until on repeated trials on small quantities of ore, taken out with a ladle from different parts of the furnace, no more sulphurous smell can be perceived; but rather a smell of muriatic acid, denoting the decomposition of the salt. The ore is then drawn out through the front door of the furnace, into a sort of iron

barrow on two wheels, in which it is taken to a convenient place to cool; and the process is repeated on another portion of ore, as before described.

This preparatory operation is altogether of such importance in an amalgamation work, that I cannot too strongly impress the necessity of the strictest attention to it; and a few observations, in addition to those I have before made, may not be superfluous.

The chemical changes that take place during the process are the following. In the first part of the operation, the sulphur becomes acidified, and from there being no fuel to be decomposed during the burning process, the whole of the atmospheric air that passes through the furnace is available for that purpose. A portion of the acid thus formed, unites with the metals in the ore, and forms sulphates. These are again decomposed on the heat being increased, when the sulphuric acid unites with the base of the salt, and forms a sulphate of soda, or Glauber salt; whilst the muriatic acid thus liberated attaches itself to the silver, for which it possesses a strong affinity, and forms muriate of silver. By this means, the silver contained in the ore is detached from its natural combinations, and instead of being in the state of a sulphuret, as in the raw ore, it is, by being exposed to the fire in conjunction with salt, converted into a muriate.

The necessity of attention to the quantity of sulphur contained in the ores mixed as I have before described, is easily understood, on considering the nature of the chemical change to be effected: for it is evident, that if there should not be a sufficient portion of sulphur to form the quantity of acid necessary to decompose the common salt, the silver will not be converted into a muriate, and consequently it will not be suited to the process of amalgamation. On the other hand, it is most material that the whole of the sulphur contained in the ore should be acidified, or expelled; for if any portion of it remained in combination with the silver, the union of that metal with the mercury would not take place.

The calcined ore, as I shall call it by way of distinction, is filled from the depositing place, into small wooden boxes, each containing 1 cwt. Six of these are placed in a square case, and raised to the upper story, through an inclosed shaft, by means of a windlass placed under the roof of the building. The ore is here passed through riddles or screens, in order to separate any pieces that may have been imperfectly calcined, from its having caked together.

The workmen carry the ore in boxes up the steps, and at the landing place empty them into the opening, which is furnished with a cover; from whence it rolls down over two inclined screens placed right and left, the holes of which are about half

an inch square. The coarse parts fall into moveable boxes; the fine particles, or what passes through the screen, fall into the cases, the opening to which is on the opposite side to the stairs. This machine is inclosed in a wooden case, and is surmounted by a funnel or chimney, which communicates with a receiving chamber immediately under the roof of the building. By this means, any fine particles that fly off during the operation are collected, and the workmen are not inconvenienced.

The coarse stuff from the upper screen is broken on a table placed near it, and being mixed with two per cent. of salt is again calcined. The finer parts that pass through the riddles fall into a hopper placed over a moveable screen on the under floor.

The holes in the grating of the screen are of two sizes; those in the upper part being somewhat smaller than those in the lower. The calcined ore falling upon the higher part of the screen, the finer particles pass through; those that are coarser roll down, and some pass the larger apertures of the lower part of the screen; whilst that portion which is too large for either, and which is trifling in quantity, rolls entirely over the screen, and falls off at the bottom. Thus three sizes of stuff are obtained. The coarsest part, which does not pass through the screen, is treated in the same way as the coarse stuff from the upper riddle. The finer parts are passed through wooden pipes to the lower floor, and are there ground in granite mills which are precisely similar in construction to those used for grinding corn. The ore which passes through each part of the screen is ground separately, the particles being of an uniform size.

After being ground, the ore is passed through a fine sieve; and being now reduced to an impalpable powder, is fit for the process of amalgamation.

The object in reducing the ore to so fine a powder is, that the mercury may act on every portion of it, so as to extract the smallest particles of silver it may contain. The calcination of the ore renders it more frangible, and therefore facilitates the operations necessary to bring it to the state just described. The Freyberg works contain two coarse and two fine screens, and 14 pair of grinding stones.

(To be continued.)

Article XI.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1825. Part II.

This part of the Royal Society's Transactions contains a somewhat unusual number of important as well as extended papers, in many distinct branches of philosophical inquiry. To give a satisfactory account of every paper would be impracticable: we must, therefore, confine our notice of some to a statement of their general object, in order to allow room for a more complete analysis of others.

X. *On the Anatomy of the Mole Cricket.* By J. Kidd, MD. FRS. Reg. Prof. of Medicine in the University of Oxford.

The venerable entomologist who has just brought to a conclusion the "Introduction" to his favourite science, in which, with his friend Mr. Spence, he was so long and so meritoriously engaged, in his Introductory Address, explanatory of the views of the Zoological Club, delivered at the foundation of that Society on Nov. 29, 1823, as already recorded in the *Annals*, expressed himself in terms of some regret with regard to the state of the Comparative Anatomy of the Invertebrate Animals amongst our naturalists. "France," he observed, "in which this science has attained to its acme, can boast of her Cuvier, Savigny, Marcel de Serres, De Blainville, Chabrier, and others; Germany of her Blumenbach, Ramdohr, Treviranus, Herold, and a host besides; Italy of her Malpighi, Spallanzani, Scarpa, and Poli; Holland of her Swammerdam and Lyonnet; but the only boast of Britain, an illustrious one indeed, *nec pluribus impar,* is her Hunter; and even he, if my recollection does not fail me, employed his scalpel chiefly on the higher orders of animals." Whether this reproach, from authority so high, has roused the attention to the subject of those who study the second grand division of animated nature, or whether we are to ascribe the results we are about to mention to the general stimulus which has of late been given to zoological science in general, in this country, we know not; but certain it is, that our anatomy and physiology of the *Invertebrata* have received some very important contributions since Mr. Kirby's address was pronounced. Among the foremost of these is Mr. W. S. Macleay's Anatomical Observations on the *Tunicata,* a natural group represented by the Linnæan *Ascidia;* and which completes the circle of affinity between the lowest subregnum of the animal kingdom, the polype *Acrita,* and the acephalous or bivalve *Mollusca.* This was published in the last part of the Linnean Transactions; and in the same rank, though perhaps less rigorously scientific in its

form, we are disposed to place Dr. Kidd's paper on an extraordinary and interesting species of the *Annulosa,* in the Philosophical Transactions now before us. Without further preface, then, we proceed to give the results of Dr. Kidd's researches; and in order to make room for the copious extracts their interest demands, giving the descriptions of the less important organs in a compressed form.

After some general remarks on the natural history, habits, and food, of the Mole Cricket,* the author describes, in the following terms, its "*external characters.*"

"Destined like the common mole to live beneath the surface of the earth, and to excavate a passage for itself through the soil which it inhabits, the gryllotalpa is furnished like the mole, with limbs particularly calculated for burrowing; with a skin which effectually prevents the adhesion of the moist earth through which it moves; and with exactly that form and structure of body, by which it is enabled to penetrate the opposing medium with the greatest ease. At the same time, in order to prevent the necessity of its excavating a track so wide as to admit of the body being turned round in case of a desire to retreat, it is endued with the power of moving as easily in a retrograde as in a progressive direction; and, apparently to perform the office of antennæ, which warn the insect of approaching danger in its progressive motions, it has two appendages, which might not improperly be called caudal antennæ, evidently calculated to serve a similar purpose during its retrogade motions; particularly as they are furnished with very large nerves. The indifference with which the insect is disposed to move in either direction is manifested by the following experiment: if you touch it towards the head, it retreats; if towards the other extremity of the body, it advances.

"The general colour of the animal is such as indirectly to serve as a protection to it, being nearly of the same hue as the vegetable mould in which it lives; so that it is not very readily distinguished upon being first turned up to view; and its safety seems to be still farther insured by the appearance of death,

* This appropriate name, we may observe, seems to have been first bestowed upon the animal, by our old entomologist Mouffet; for in his *Insectorum sive Minimorum Animalium Theatrum,* which was published at London in 1634, and from which, though the earliest work expressly devoted to insects, valuable information may sometimes be derived, we find the subjoined remarks commencing "*Cap.* xxiv. *De Gryllotalpa.*"

"Liceat hic quæso nobis præ nominum inopia ὀνοματοποιεῖν.

"Bestiolam quam expressimus, Cordi Sphondylis, Dodonæi vera Buprestis est: perperàm uterque nominant et nullo jure. Sphondylis enim alas non habet, hoc Insectum vides alatum. Buprestis Cantharidi similis apud omnes dicitur; hoc vero animal nec figura, nec colore, nec magnitudine quicquam eo accedit; ut taceam elytrorum hic absentiam, quibus Cantharides carere nemo sanus contenderit. Gryllum dicimus, quia eundem cum Gryllo stridorem nocte appetente facit. Talpam, quia terram continuo fodit. Belgis *Weemol,* Anglis *fenkricket, evechurrē,* atq. etiam Churrworme dicitur." The name appears to have been speedily adopted into general use; for in the *Musæum Tradescantium,* published only twenty-two years afterwards, in 1656, among the *Insecta* is mentioned, "*Grillo talpa* tardi-gradus."

which, in common with many other insects, it assumes when suddenly disturbed. This stratagem, for so it may be called, appears to be most decidedly practised by the animal while in captivity; and if thrown at random out of the vessel in which it has been confined, however unnatural the posture may be into which it has been thrown, it remains as it were in a state of catalepsy during half a minute or more; the first indication which it gives of recovery from this stupor, invariably consists in a motion of the extremity of the antenna.

"The general colour of the insect is a dusky brown, passing either into a reddish brown, or into an ochry yellow; those parts being of the darkest colour which are most exposed to view when the animal is moving in the open air. Every part of the body is to a greater or less degree covered by a kind of down, which seems to be the efficient cause of its capability of repelling moisture; which capability is so remarkable, that when the insect is plunged under water, it appears as if cased in silver, or some bright metallic covering; this appearance being evidently derived from a stratum of air, interposed between its body and the surrounding liquid. This down not only serves to repel the adhesion of any moist substance to its body, but also facilitates the motion of the animal, by lessening the degree of friction which would otherwise take place; and it is owing to the same circumstance that there is an unusual degree of difficulty in retaining a sure hold of the insect, even when dead; but more especially when alive, and struggling against detention. The degree of force which it commonly exerts on such occasions is very remarkable; and, from the sensation produced, may easily be supposed to be what Rösel says it is, equal to the counterpoise of two or three pounds. The skin or covering of the insect is in some parts nothing more than a thin membrane; in other parts it resembles soft leather; and sometimes equals horn or even shell in its degree of hardness."

Head. Upper part and sides a thick, hard, horny case, containing the motor muscles of the jaws: in order to strengthen it, two firm bars run transversely across the bottom both of the anterior and posterior margin, which are united by a still stronger bar running longitudinally from the middle of the one to that of the other.

"The antennæ, which are situated near the articulation of the mandibles, consist of a great number of minute segments; resembling beads of a circular form: the number of these beads, which varies in different instances, is usually from 100 to 110; rarely more or less: but it is worth noticing that in examining the two antennæ of the same individual, I sometimes found the number of beads greater in one than in the other; and as the terminal bead differs in its form from all the rest, the result of the examination is less open to doubt than it would otherwise have been. Each bead is united to the one that precedes and

the one that follows it by means of a soft, white, very flexible membrane; in consequence of which, and of the number of the joints, the insect can move and bend the antennæ with great facility in every direction, excepting at the very root: there the motion is confined by a ridge that only admits of its being directed from behind, forwards, or *vice versâ*.

"The anterior edge of each bead is fringed with bristly hair; which, surrounding the joint that connects it to the following bead, gives to the whole, when viewed by a magnifying lens, the appearance of a sprig of equisetum. The beads are upon the whole larger, in proportion as they are nearer to the origin of the antennæ; but here and there, and without any regularity in the variation, one of the beads is either much larger or much smaller than those in the vicinity.

"Whatever be the primary use of the antennæ and palpi, on which subject entomologists are not agreed, their general importance is allowed by all; and is evinced in the particular instance now before us by the extraordinary attention bestowed upon them by this insect. Those who may be led to watch its habits, will repeatedly observe the antennæ bent forwards and downwards, by a curious application of the fore-legs towards the mouth: and then by a regulated motion, not unlike that by which the resin is applied to the bow of a violin, they are passed between the maxillæ: in order, as it would appear, either to moisten the organs, or to disengage from their surface, particles of dust or other extraneous substances which may have accidentally adhered to it. With a more rapid motion the insect from time to time dresses, if I may use the expression, its palpi; bending them inwards and brushing the surface of their extreme parts by a frequent application of the maxillæ. A similar care of the antennæ and palpi is observable in the gryllus viridissimus; with the additional circumstance, that that insect very often passes between its maxillæ the curiously padded surfaces of its feet, much in the same manner as a cat licks its paws.

"*The Eyes.* The gryllotalpa has two compound eyes, as they are called, and two ocelli or stemmata. Latreille uses this expression "ocellus medius subobiteratus;" from which it may be inferred that he supposes the ocelli to be three in number; but after the most careful examination I have not been able to discover more than two. The compound eyes are situated immediately behind, but a little exteriorly to the antennæ: the corneæ of these eyes, which are large in proportion to the size of the head, are segments of a sphere; flattened however on the inner side so as to present a vertical plane surface to a similar plane surface in the opposite eye; and it is remarkable that this part of the cornea, and the mere margin of the rest of it, are the only parts capable of freely transmitting light: all the remaining portion is covered, on the interior surface, by an opaque pulpy

membrane, or pigments of a mulberry colour; yet the portion obstructed by this pigment is in itself nearly as transparent as flint glass: it is studded over on the interior surface with numerous depressions of a circular form, which, being very closely set together, give it a reticulated appearance.

"The stemmata are placed between the middle of the compound eyes, so as to be rather further from each other than from the eye of the same side. They are not so large as a very minute pin's head, of a lenticular form, perfectly transparent, but not quite colourless, resembling particles of very pale cairngorum quartz. In two instances I have found only one of the stemmata, without any trace of the other. An anomaly somewhat of the same kind has been observed by the father of my friend Dr. Ogle, of this University, in the case of a man; on one side of whose breast the usual rudiments of a mamma were entirely wanting.

"With respect to the small quantity of light admissible through the corneæ of the eyes of the mole-cricket, it is apparently sufficient for the purposes of an animal living almost constantly underground. The spherical form of that part of the corneæ which is itself incapable of transmitting light is probably intended, as was suggested to me by Mr. Whessel, to whom I am indebted for the principal drawing which accompanies this paper, as a protection for the vertical transparent portion."

Thorax. An irregularly cylindrical, anteriorly conical, strong horny case, its upper portion and sides covered with fine down, almost entirely occupied by the muscles of the fore-legs, which are attached to an almost bony septum of a complicated form, that divides the cavity longitudinally.

We pass over the general account of the *abdomen* to that of its last segment, which sends out from each side of its upper surface two "*caudal antennæ,*" * as Dr. Kidd denominates them, of a tapering form, and differing essentially in structure from those of the head, as they are not jointed, excepting at their very commencement: they are furnished with short closely set hairs, interspersed with long single hairs. They are evidently very sensible, and serve probably to give the animal notice of any annoyance from behind; they are partially hollow throughout great part of their extent, and muscles may be traced into them from the adjoining part of the abdomen.

"*The Legs.* The anterior legs passing out from under the hind part of the thorax, advance by the side of the head in a direction parallel to each other, which is their natural position while the animal is at rest. I should deem it a servile adherence

* As these organs are certainly analogous to the *cerci* of the *Blattidæ* and some other families of insects (though they differ from them in being conical and devoid of joints), that appellation, we conceive, would have been more appropriate than the one Dr. Kidd assigns them.

to system were I to describe the parts composing these legs by the terms strictly indicative of the order of their succession; for thus that part which answers so eminently to the character of a hand must be called the tibia. I shall beg leave therefore to state principally that the fore leg of this insect consists of three main parts, with a lateral appendage attached to the last of them. The two first of the three parts bear some general resemblance to the claw of the crab; being short and thick, for the purpose of affording room for powerful muscles, intended to move the last part; which is the immediate instrument employed by the animal in burrowing.

"It might I think be asserted, without the fear of contradiction, that throughout the whole range of animated nature, there is not a stronger instance of what may be called intentional structure, than is afforded by that part of the mole-cricket which I am now to describe.

"The natural and constant position of this member is worth noticing; the palm, as it may be called, facing outwards, and the claws ranging not in a horizontal but a vertical line, so that none of them but the lowermost, and not even this necessarily, touches the surface on which the animal is walking. Accordingly the insect does not make much use of its fore-legs in walking; and, if irritated, it advances towards you with these legs elevated, in a menacing attitude as it were; not unlike the corresponding attitude of the insect called the mantis. The form of the hand is that of a triangle; the base of which is formed by the four claws, while the apex is situated at the joint connecting this with the preceding part; by which form and disposition two important objects are gained; for the joint is thus capable of a much greater extent of motion than it could have possessed, had the articulating surface been more than a mere point; and at the same time, the greater extent of the base enables it to act with more powerful and more rapid effect than could have been otherwise produced. The four claws, which form this base, constitute the proper burrowing instrument; and their shape and structure are beautifully adapted to the purpose; for instead of being covered with down or hair, like all the rest of the limb, they are hard, and have a perfectly polished surface; doubtless in order to prevent as much as possible the adhesion of the earth through which the animal is to make its way; they have each of them sharp but strong points, which proceeding from a broad base are thus rendered more effectual. In each also of the claws one of the edges is sharp, while the other is comparatively blunt; and all the cutting edges, as also the terminating points, are directed downwards. Their outer surfaces are slightly concave both in the longitudinal and transverse direction; so that all together they form a scoop as it were, by which the earth that has been scraped off by the points is moved out of the

way. They are also each of them divided longitudinally on their concave side by three or four slight ridges; so that, though highly polished, their surface is not absolutely smooth; and thus being concave and uneven, they are more apt to retain particles of the excavated earth; which, by filling up the indentations of the claws, would necessarily impede their due action. To obviate this inconvenience, an exceedingly curious instrument is attached to the upper part of the concave surface of this member: this instrument consists of two claws, closely resembling those already described, having by their side a small brush as it were, which terminates in two spines. These two claws, together with the piece bearing the spines, arise from a single piece, or handle, which is articulated in such a manner, as to move in a plane parallel to that in which the four claws are placed: but in a direction opposite to that in which they are moved: they are also placed in such a manner that their points and cutting edges are opposed to the points and cutting edges of the true claws; and hence the two parts, thus opposed to each other, act like the blades of a pair of shears. When first I considered this mechanism, and remembered that in the localities where I had found the animal, the earth was frequently traversed by fibrous vegetable roots, which must necessarily retard its progress, I supposed that it used this instrument as a pair of shears to cut through those fibres. It is Rösel's opinion, however, that the instrument is intended to clear the true claws of the dirt that may from time to time collect upon and clog them; and unless both opinions be true, Rösel's appears the more probable. But I have not yet concluded the account of the curious mechanism of this member; for the brush which has just been described, has only such an extent of motion as enables it to clear the two uppermost claws, or at most the three uppermost: the two lowermost however may effectually be cleared by a kind of feathered spur, which, arising from the further extremity of the joint answering to the femur, proceeds directly towards the lowest part of the burrowing instrument, and is easily made to sweep over the surface of the two last claws by bending the intermediate joint, the only difference in its mode of action being, that it passes over their inner instead of their outer surface.

"The middle pair of legs, which are the smallest of the three pairs, arises from the under part of the first segment of the abdominal division: they pass out from the body at right angles to the abdomen, and usually are seen in that direction whether the animal be in motion or at rest. They consist each of four parts; a very short coxa, a femur and tibia nearly equal in length to each other, and a tarsus, which consists of two long and an intermediate short joint; the last joint terminated by two curved spines. There are several sharp, hard, straight spines near the

angle made by the union of the tibia with the tarsus; some of which being directed downwards, give the insect a firmer hold in walking.

"The hind legs bear a general resemblance to the middle legs; but the coxa, femur, and tibia, the femur especially, are much larger and stronger; the relative position of the parts with respect to each other is the same as that of the middle legs; but their general direction, instead of being at right angles to that of the abdomen, is parallel to it. In addition to several sharp spines placed about the joint of the tibia and tarsus, and directed downwards as in the middle legs, there are four or five others placed at the back of the tibia near its lower extremity, and pointing slightly downwards. The structure of the tarsus scarcely differs from that of the middle leg. These hind legs are evidently the great instruments of progressive or retrogressive motion."

Omitting the description of the wings, and proceeding to that of the *digestive organs*, we find the author combating the opinion that the gryllotalpa ruminates, and stating the internal structure of the parts to destroy the probability of its truth. Its digestive organs resemble more closely those of a granivorous bird than of any other animal. One of the most remarkable is the *gizzard*, which terminates a short tube passing from the crop towards the intestines. It is scarcely larger than a hempseed, nearly spherical, consisting of a thick external muscular coat, lined by a glandular membrane, the inner surface of which is divided longitudinally into six equal parts, separated from each other by two dark-brown horny ridges; each division is furnished with three series of serrated teeth, of the consistence, and nearly of the colour of tortoise-shell, running from the top to the bottom. There are 15 teeth in each of the three series of the six divisions, making in the whole 270. Four of these divisions terminate in a tapering membranous appendage, consisting of a natural fold, which serves to convey onwards any fluid particles that may have been pressed out by the action of the gizzard. This is contained in a large membranous cavity of the shape of a horse-shoe, the base of which passes across the lower extremity of the gizzard, while the sides form two enormous cæca, which ascend obliquely outwards on each side of the gizzard.

The *jejunum*, great intestine, and liver, we have space only to mention, and proceed to extract Dr. Kidd's account of

"*The Blood.* Upon wounding the animal in almost any part of the body, even in cutting off a portion of the caudal antenna, there oozes out a very clear thin fluid of a bright honey-yellow colour; having sensibly alkaline properties, and coagulating either by heat or by the addition of alcohol. A quantity of this fluid, weighing 1·85 grain, being evaporated under an exhausted receiver, in which was placed dry muriate of lime, left a solid

residuum of a bright golden yellow colour, which weighed 0·25 grain; this residuum was brittle, and had the general properties of solid albumen. The foregoing characters render it highly probable that the yellow fluid distributed through the body of the insect, resembles in its nature the serum of common blood, and there can be no doubt, arguing physiologically, that this yellow fluid is the blood or nutrient juice of the animal. I wish I could as satisfactorily show the means employed by nature to distribute this fluid through the system of this and other animals of the same class; for, though I cannot hope to discover what more experienced and skilful anatomists have sought in vain, a heart, namely, and a system of circulating vessels; yet I cannot subscribe to their opinion, that the blood transudes through the coats of the intestines, where of course it must be primarily formed, and thence passes, as through the pores of a sponge, to every part of the body. Both Cuvier and M. Marcel de Serres completed a very elaborate set of experiments for the purpose of ascertaining whether the dorsal vessel of insects sends out any lateral branches which might serve the purpose of a circulating system, or whether any other distinct circulating system exists; but they have entirely failed in their endeavours; and I feel assured, that where such men have failed, others will not succeed; and yet their consequent supposition that the blood is diffused through the general substance of the body, appears to me very highly improbable. It accords not with the general character of those means by which nature usually produces its effects; there is too little of art and contrivance, if I may use such terms, on such an occasion, in the mode supposed to be employed. Even in the formation of mineral crystals, which are unorganized bodies, the attraction by which the component particles are aggregated is regulated by laws the most systematically framed and observed: and whoever has viewed with any attention that wonderful monument of human industry and sagacity, the Anatomical Museum of John Hunter, and has there seen the proofs of a sanguineous circulation in animals of an order so low, that they can hardly be said to have any specific form or substance, will almost necessarily be disposed to expect a similar provision in a class of animals, whose general structure is so elaborately and beautifully organized as that of insects."

Organs of Respiration. Ten stigmata are very distinctly visible on each side of the body. The first in order beginning from the head is situated very near the lower part of the posterior ridge of the thorax. This is apparently connected with all the tracheæ, both of the thorax and the head itself. It differs remarkably in size and form from all the rest; for instead of being a mere dot or point, it is an elongated fissure, bounded by two horny lips. The second stigma, which somewhat resembles in

form, though of less extent than the preceding, is situated immediately behind the root of the middle leg; the third, which is still less than the second, is situated immediately behind the root of the posterior leg; from the fourth to the tenth inclusive, are situated near the terminations of the corresponding dorsal segments of the abdomen.

The stigmata, taken generally, are not the terminations of single tubes; very frequently two and even more tracheæ originate from the same stigma; and very soon after the commencement, one or even two of these tracheæ subdivide into numerous branches, which follow as nearly as may be the direction of the original tubes.

"It is generally understood, that the tracheæ of insects penetrate each organ and every part of the body; and certainly the case is such in the instance before us. Thus, in that brush of capillary yellow tubes supposed to constitute the hepatic system, the total number of which amounts to 150 or 200, there is reason to believe that each tube is accompanied by a distinct trachea coiled round it in a long spiral. Again, the two medullary cords which connect the several ganglions of the nervous system, are in their natural state united together by means of the branches of a tracheal tube which runs between them; a similar tube being attached to the exterior edge of the cords; and the surface of what may be called the brain of this insect is as beautifully characterized by the ramifications of the tracheæ which pervade it, as the surface of the pia mater of the human brain by the blood vessels which penetrate that membrane in every direction.

"In meditating on the difficult problem of the sanguineous circulation of insects, it has forcibly occurred to me, that the tracheæ may possibly be the instruments of such a circulation; absorbing the blood or the chyle in the first instance from the internal surface of the alimentary canal, and thence conveying it to the various parts of the body; nor is this opinion, however improbable it may appear, entirely gratuitous. No difficulty, I apprehend, attaches to the supposition that such an absorption may take place; seeing that innumerable minute ramifications of the tracheæ penetrate the intestinal canal in every part; nor does there seem any difficulty in admitting that the insect may, by the power of exhausting the air from individual tracheæ, draw on the absorbed fluid towards those two lateral tracheal tubes, which are apparently a general medium of communication between all the other tracheæ of the body. And when once the blood has reached this supposed point of its course, it is manifest, that by whatever means the air itself is forwarded from the same point to the most distant parts of the body, by a modification of the same means, the blood may be forwarded to the same part; and the elegant proposition of Cuvier, that "the blood

being incapable of going in search of the air, the air goes in search of it," will still remain inviolate.

"If it should be argued that the tracheæ are not found charged with blood after the death of the animal, it may be answered, that neither are the arteries in the higher orders of animals found charged with blood after their death. However, I have actually seen some of the ramifications of those tracheæ which are connected with the cæca distended with a fluid of the same colour as that found in those organs; and though I have only witnessed this fact in two instances; yet such a fact, even singly taken, must be allowed to be of considerable importance.

"Of one thing I am certain, that after careful observation, I have never found the abdominal viscera, I will not say bathed, as some authors of credit have expressed themselves, in the nutrient fluid which is supposed to have transuded through the coats of the intestines; but I have not even found them lubricated by a greater proportion of moisture than lubricates the intestines of the higher classes of animals.

"There is another difficulty which occurs to the hypothesis of the transudation of the chyle through the coats of the intestines; for, if the blood be conveyed to the several parts by previous general diffusion through the interior of the body, and then by absorption into the substance of particular organs, as the hepatic tubes, the vesiculæ seminales and the ovaries; how does it happen that the bile, for instance, does not transude through the coats of the same vessels, the pores of which have admitted the blood from which it has been formed? It may be answered, that the alteration which the blood undergoes in the several organs changes its properties to such an extent, as to render it incapable of repassing through the pores which admitted it. I cannot of course presume to say that such is not the case; and I am aware that many entomologists will be surprised at, and perhaps disinclined to listen to, the opinion here advanced with respect to a sanguineous circulation in insects; but I nevertheless hope that the opinion will not be rejected without some previous attention to it. With regard to the dorsal vessel of the gryllotalpa, which in this, as in other insects, has been supposed to stand in the place of an arterial heart, I have very few observations to offer. It does not agree in its form with the description commonly given of this mysterious organ; for though it diminishes in diameter as it approaches the head, this is by no means the case towards the other extremity of it. I have not yet completely succeeded in tracing this vessel to its anterior extremity; because as it approaches its termination in that direction, it becomes so delicate as to have hitherto broken under dissection before I arrived at the extremity of it. Towards the opposite extremity, it gradually becomes larger from the

centre of the body, and terminates apparently in a cul de sac about the last segment but two of the abdomen."

Dr. Kidd next describes the muscles and nerves of the gryllotalpa, the latter arising from a series of nine ganglions; and he gives the following account of the brain.

"The brain differs in colour from the ganglions, being of a pale brownish pink, instead of a cream colour, and in size it far exceeds the largest of the ganglions. It consists of two hemispheres, separated by a fissure, from each of which pass out four processes: the first of these processes unites as above described, with a process from the ninth ganglion, to form the nervous collar of the esophagus; the second passes to the root of the antenna; the third, which may be called the optic nerve, passes towards the inner surface of the cornea; and at its extremity swells out into a fringed coronet of an orange red colour; the fourth process, the extremity of which is also of an orange red colour, proceeds to the ocellus or stemma of the corresponding side.

"The upper surface of the brain is covered by a mass of soft substance somewhat resembling loose fat."

The paper is concluded with an account of the generative organs of each sex, some observations on the casting of the skin, and on the organ of sound, and the dimensions of the full-grown mole-cricket.

"*The Sexual Organs of the Female.* These organs consist o. two ovaries, which occupy a considerable portion of the upper part of the abdomen, and terminate by a narrow duct in a common cavity or uterus, which opens externally under the posterior edge of the last segment but one of the ventral surface of the abdomen. Behind the uterus is an oblong white body, which, originating from a cul de sac, and then doubling on itself in the form of a slender tube, terminates in the uterus. The contents of this body resemble a thin white paste. The ovaries are irregularly pear-shaped, and consist of a transparent membrane irregularly convoluted, through which the ova, enveloped in a gelatinous medium, are easily distinguished. In the same ovary the ova are frequently of different sizes and colours; those which are the largest, and which I suppose to be impregnated, are of a brownish yellow colour; they resist a considerable degree of force before they burst, and the contents when pressed out melt as it were into a soft jelly, leaving a tough membrane which enveloped them. The smaller ova are of various sizes and of nearly a white colour, and of a much more slender and compressed form than those which I have supposed to be impregnated. This difference in the degree of maturation corresponds with a fact stated by Rösel, that the mole-cricket does not deposit all the eggs of the season at one time. In a few instances I found two or three ova which had entered the narrowest part of the duct, and were very near the uterus; and from the appearance of these, which may fairly be supposed to

be, if not impregnated, at least in a state fit for impregnation, I have ventured to derive the character of the impregnated ovum.

" *The Sexual Organs of the Male.* I had dissected several male gryllotalpæ before I was fortunate enough to meet with the sexual organs fully developed; and while I had as yet met with only one animal bearing the character of full developement, I was not certain whether I judged rightly of the natural state of those parts; or whether their uncommon degree of enlargement were not the effect of disease—the disproportion in size between the state in which they had hitherto occurred, and that to which I now allude is so enormous. However, subsequent dissections presenting the same phænomena, I have no scruple in considering them as indicating full developement.

" The testicles of the male are situated similarly to the ovaries of the female, and are not very unlike in general appearance to the ovaries of young females; they differ however in being divided pretty deeply into several unequal lobes, the free extremities of which look towards each other. They send out each a very fine capillary tube or duct; which, descending towards the rectum, is in one part of its passage convoluted on itself so as to resemble the human epididymis partially unravelled.

" The excretory duct above described terminates at the bottom of a thick pouch, which is situated between the rectum and the ventral integuments, and in form is not very unlike, though larger than the uterus, opening externally, as the uterus does, under the posterior margin of the last but one of the ventral segments of the abdomen.

" The interior mechanism of this pouch is extremely curious; for in the upper part there is contained an apparatus somewhat in the shape of a coronet, of the colour and hardness of tortoise-shell; and at right angles to the centre of this there is fitted a similarly hard and horny substance (in shape resembling a short flat club), which descends towards the external opening of the pouch.

" Behind the pouch are situated one on each side, two oblong white bodies, which are twisted into three spiral coils, and then terminate by an inflected tube at the upper and back part of the pouch. These bodies evidently answer to the vesiculæ seminales of insects in general; and resemble in their external character, and in their white pulpy contents, that oval body which is placed at the back of the uterus. There is also another pair of vesiculæ seminales, as is frequently the case in insects, situated exteriorly to the former; more slender in form, also and much more convoluted, which apparently terminate near the points where the ducts of the testicles terminate. In the instances of full developement these bodies are enlarged to six times their usual size. Under the circumstances of full developement there is also found, though scarcely perceptible under imperfect developement, a large spherical mass, resembling a

ball of eider down, situated immediately at the anterior edge of the pouch above described, and continued on from its substance.

"The examination of the mole-cricket has added, as appears from the description of the parts, another exception in the case of the female as well as the male, to the general statement, that in insects the sexual organs pass out by the anus. Cuvier mentions, as the only exceptions to this law, the Juli and Libellulæ."

"*Dimensions of a full-grown Mole-cricket.*

	Inches.
Length of the body from the extremity of the lip to the extremity of the vent	2·0
Length of the head	0·165
——— thoracic division	0·5
——— abdominal division	1·33
Breadth of the thorax	0·5
——— abdomen	0·5
Length of the antennæ of the head	0·825
——— caudal antennæ	0·666
——— whole alimentary canal	2·0
——— esophagus	0·5
Length from the crop to the great intestine	0·5
Length of the great intestine	1·0"

XI. *Further Observations on Planariæ.* By J. R. Johnson, MD. FRS. (See *Annals* for April, 1825.)

XII. *On the Influence of Nerves and Ganglions in producing Animal Heat.* By Sir Everard Home, Bart. VPRS. (Communicated by the Society for the Improvement of Animal Chemistry.)

XIII. *An Essay on Egyptian Mummies; with Observations on the Art of Embalming among the Ancient Egyptians.* By A. B. Granville, MD. FRS. &c.

The problem of the method really pursued by the ancient Egyptians in the preparation of their mummies, which has so long been an *opprobrium Antiquariorum,* appears to have been completely solved by Dr. Granville. We have already given some notice (see *Annals* for June, 1825), of his researches on the subject detailed in this paper, from which, as introductory to the extracts subjoined, our readers will acquire a correct knowledge of the principal results of the investigation. We commence with the external appearances of the mummy which Dr. G. dissected.

"The mammæ must have been large during life, for they were found to extend as low down as the seventh rib, against which they are closely pressed by the arms passing over them. But on lifting the latter, the breasts themselves were raised with little exertion. Of these organs there remain, of course, little more than the integuments, which are of considerable thickness, and exhibit the nipples with their surrounding areolæ in a perfectly distinct manner.

"The head is closely shaved; the short hair, which is of a brown colour, can be felt on passing the hand over it; and on close inspection may be distinctly seen. Externally the cranium appears not to have been disturbed in any way. The eyelids were in close contact. The nose has been flattened down towards the right cheek by the action of the bandages. The lips, from being retracted, allow the teeth of the upper and lower jaw to be seen, perfectly white, and in a sound condition. The arms are crossed over the chest, the fore arms directed obliquely upwards, towards the extremities of the shoulders. The fingers of the left hand alone were bent inwardly, the thumb remaining extended."

"Following up my description of the external appearances of our mummy, I have to remark that the inferior extremities were brought together in close contact at the knees and feet, which latter were kept in that position by a contrivance similar to that which obtains to this very day in most parts of Europe, of fastening the two great toes by means of a piece of rag or tape.

"Numerous and deep wrinkles appeared on the integuments of the abdomen, denoting that before death, this part of the body must have had very considerable dimensions; a conjecture, the correctness of which subsequent inquiries have completely demonstrated.

"The general surface of the body is of a deep brown colour, approaching to black, and is quite dry. In parts where the larger muscles lie, as the thighs for instance, the surface feels quite soft to the touch, and the muscles yield slightly to pressure. The cuticle appears to have been removed throughout, except at the extreme points of the fingers and toes, where it can yet be seen curled up, retaining the nails, of a deep brown colour, in their situation. Some of these, however, quitted their fastening when the slightest attempt was made to detach them."

	Feet.	In.
"Height of the mummy from the vertex of the head to the inferior surface of the calcaneum	5	$0\cdot\frac{7}{10}$

Thus divided.

Length of the head from the vertex to the first vertebra of the neck	0	$6\cdot\frac{2}{10}$
Length of the back bone from the first vertebra of the neck, to the articulation of the os sacrum with the os coccygis	1	10
Length of the thigh from the centre of the head of the femur to the centre of the knee pan	1	$5\cdot\frac{3}{10}$
Length of the leg from the centre of the knee pan to the inferior surface of the calcaneum	1	$3\cdot\frac{2}{10}$
Total	5	$0\cdot\frac{7}{10}$

"The dimensions of the upper extremities and of the foot, are these:

	Feet.	In.	
Length of the arm	1	$1\cdot\frac{5}{10}$	} 2 6·
—— of the fore arm	0	$9\cdot\frac{5}{10}$	
—— of the hand from the tip of the middle finger, to the articulation at the wrist	0	7	
Length of the foot	0	$7\cdot\frac{6}{10}$	

"Now we find, on comparing the principal of these dimensions with those of the Venus de Medicis, as given by Winkelman, Camper, and others, that the difference between them is so slight, as not to deserve notice. Our mummy is that of a person rather taller. The celebrated Medicean statue, which stands as the representative of a perfect beauty, is five feet in height, like our mummy, and the relative admeasurements of the arm, fore-arm, and hand in each, are precisely similar.

"But in a female skeleton, it is the pelvis that presents the most striking difference in different races. Nothing, for instance, can be further removed from the symmetrical form, and from the dimensions of the pelvis in the Caucasian or European race, than the same part in the Negro or Ethiopian race. Of this fact, I shall be able to convince such of the Fellows of this Society, as are not conversant in these matters, by exhibiting the most perfect pelvis of a well grown Negro girl, which I prepared some years ago, in contrast with that of our mummy, which I likewise carefully dissected. When subjected to this comparative test, the pelvis of our female mummy will be found to come nearer to the *beau idéal* of the Caucasian structure, than does that of women of Europe in general, and to equal in depth, amplitude, and rotundity of outlines, the Circassian form.

"In illustration of this remark, I made the following measurements.

	In.
Greatest distance or width of the pelvis from the highest point of the ridge of the ilium on one side, to that of the other side	$11\cdot\frac{5}{10}$
Distance between the two anterio-superior spinous processes of the ilia	10
Distance between the tuberosities of the ischium	$3\cdot\frac{5}{10}$
Elevation of the branches of the ischium to join the descending branches of the pubis, and form the subpubian arch	3
Greatest elevation of the os innominatum or haunch bone, from the tubera of the ischium to the highest point of the crest of the ilia	8

Diameter of the pelvis.

Transverse, or bi-iliac diameter	$5 \cdot \frac{5}{10}$
Anterio-posterior, or sacro-pubian diameter	$4 \cdot \frac{5}{10}$
Oblique, or sacro-ilio-cotyloid diameter	$5 \cdot \frac{5}{10}$

"Not only are these the most perfect dimensions which a female pelvis can have, but they are precisely in the proportion which the longest diameter bears to the shortest, in the Venus of the Florentine Gallery, according to Camper, namely, as 46 to 34; whereas in the Negro or Ethiopian race, the proportion is 39 to $27\frac{1}{2}$, or what amounts to the same thing, the longest diameter of the pelvis of the Negro girl above-mentioned is only $3 \cdot \frac{9}{10}$ inches, while the shortest is no more than $3 \cdot \frac{6}{10}$ inches. In this respect my admeasurements agree with those given by Soemmering.

"What has just been observed of the skeleton generally, and of the pelvis in particular, applies with equal force to the form and dimensions of the head. So far from having any trait of Ethiopian character in it, this part of our mummy exhibits a formation in no way differing from the European.

"On looking at the Plate which represents with scrupulous accuracy the contour of the head of the natural size, it is impossible not to be struck with the likeness it bears to the skull of the Georgian female represented in the "*Decas tertia Craniorum*" of Blumenbach's very instructive collection. In both we have the facial angle approaching nearly to a right angle; and the configuration of the vertex and occiput in each is such, as must attract attention for its elegance, and the indication of a something more important than mere beauty.

"It may be affirmed then, that Cuvier's opinion respecting the Caucasian origin of the Egyptians, founded on his examination of upwards of fifty heads of mummies, is corroborated by the preceding observations; and that the systems which were founded on the Negro form, are destroyed by almost all the recent, and certainly the most accurate investigations of this interesting subject. It is a curious fact, which has been noticed by more than one traveller, that whole families are to be found in Upper Egypt, in whom the general character of the head and face strongly resembles that of the best mummies discovered in the hypogei of Thebes; and not less so, the human figures represented in the ancient monuments of that country."

"An incision having been made into the parietes of the abdomen, just below the ribs, and continued down to the hip bone, on both sides, and carried along the margin of the pubis, the whole of the integuments and muscles were removed, so as to expose that cavity completely to view. The objects which then presented themselves were a portion of the stomach adhering to

the diaphragm, the spleen much reduced in size and flattened, attached to the super-renal capsule of the left kidney, and the left kidney itself, imbedded in, but not adhering to the latter, and retaining its ureter, which descended into the bladder. This, as well as the uterus and its appendages, were observed in situ, exhibiting strong marks of having been in a diseased state for some time previously to the death of the individual. Fragments only of the intestinal tube could be found, some of them of considerable dimensions, and among them part of the cœcum, with its vermiform appendix, and portions of the ilium. Several large pieces of the peritoneal membrane were likewise observed."

"The cavity of the abdomen being emptied of all its contents, Icontinued the circular incision back to the spine, which I divided at the first lumbar vertebra. I next sawed off the thighs a few inches from the hip, and dissected carefully all the soft parts from the pelvis, so as to ascertain the condition and dimensions of this important part of the female skeleton. In performing this last operation, which occupied me two hours a day for nearly a week (some medical or scientific friends being present at each sitting), we could not help being struck with the remarkable degree of preservation of the muscles, such as had never before been noticed in Egyptian mummies, and such as to admit of their being separated from one another, as readily as in the dissection of a recent subject. Nor was the perfect condition of the articulatory membranes and ligaments less surprising, which allowed us to impart to the great articulation of the thigh with the ilium, its various movements, a circumstance seldom observed, even in modern preparations of the pelvis.

"The cavity of the thorax was next examined, and this I effected without disturbing the anterior portions of the ribs or breast bone, by simply detaching the diaphragm all round, and bringing it away. It was found that the pericardium, which adhered partially to the diaphragm, came away with it, and that a laceration had taken place at the same time in that sac.

"This circumstance denoting that the heart was present, I introduced my hand to remove it, when it was found suspended, *in situ*, by its large blood vessels, in a very contracted state, attached to the lungs by its natural connexions with them. The latter organs adhered throughout their posterior surface to the ribs, and were brought away altogether in as perfect a state as could be effected.

"The last cavity examined was that of the cranium; for this purpose it was sawed in two, horizontally, and when thus opened, it was ascertained that the brain had been removed through the nostrils; the plates of the inner nasal bones having been destroyed in the operation by the instrument employed, as evidenced by the state of those parts. It is a matter of no little surprise how, under circumstances of so much difficulty, the

operators could have contrived to remove every vestige of the membranes investing the brain, one of which is known to adhere firmly in most subjects to the inner surface of the superior cranial bones. There can scarcely be a doubt but that some injection had been thrown into the cavity in question, to clear it out in so perfect a manner; for no instrument could have effected such a purpose. A black resinous substance, but in a small quantity, was found adhering to the inner surface of the occipital bone, which must have been thrown in quite hot, as it had penetrated through, and burnt partially, the superior part of the lambdoidal suture through which the liquid escaped, so as to be now seen extravasated under the scalp. But how this liquid resin was thrown in, and for what purpose, it is not easy to conjecture. It could only have been made to penetrate through the opening which had previously been made in the ethmoid bone, to extract the brain; and if so, it is difficult to conceive in what manner it was made to reach the spot it now occupies without having adhered to any other intermediate portion of the cranium. It was remarked, at the time of opening the head, that its inner surface was studded with small crystals of what appeared to be an animal substance, resembling *steatine*.

"The last observation I have to make on the structural condition of this mummy, refers to the state of the eyes, which appear not to have been disturbed; and to the state of the mouth, which was as carefully examined as circumstances would admit, without destroying the contour and general appearance of the face. The tongue is preserved, and neither above nor below it was there found any coin or piece of metal, as recorded of some of the mummies, but a lump of rags dipped in pitch. The teeth, as I before remarked, are perfectly white and intact; nor did I observe that peculiar cylindrical form of the incisores which has been assumed by some naturalists, as one of the characters of the head in the Ethiopian race."

The following is Dr. Granville's account of the process followed by the Egyptian artists in the preparation of a mummy; as deduced by him with great accuracy, from the results of his examination of the specimen just described.

"A. Immediately after death the body was committed to the care of the embalmers, when, in the majority of cases, the viscera of the abdomen, either wholly, or partially, were forthwith removed; in some cases through an incision on the one side of the abdomen, as stated by Herodotus, and as proved by some of the mummies examined; and in others through the anus, in which latter case, the extremity of the rectum was previously disengaged from its attachments all round by the knife, and the intestines imperfectly extracted. The cavity of the thorax in the most perfect specimens was not disturbed.

"B. The head was emptied, in all instances, of its contents, either through the nostrils, by breaking through the superior

nasal bones, as in the instance under our consideration, as well as in that of the head from Tripoli, already mentioned, or through one of the orbits, the eyes being previously taken out, and artificial ones substituted in their place, after the operation, as in the instances of the mummies examined by Sir E. Home and Mr. Brodie. The cavity of the cranium was repeatedly washed out by injections with some fluid, which had the power of not only bringing away every vestige of the substance of the brain, but even of the enveloping membranes of it. Yet the liquid could not have been of a corrosive nature, else the tentorium, or that membranous floor which supports the brain, must have disappeared with the meninges; whereas it is still in existence, and does not appear to have been in the least injured. A small quantity of hot liquid rosin was then injected into the cranium.

"C. The next step taken in the embalming process was to cover the body with quick lime for a few hours, and after to rub the surface of it with a blunt knife, or some such instrument as would most effectually assist in removing the cuticle. The scalp, however, does not appear to have been touched; and care was taken also not to expose the root of the nails to the action of the alkali, as it was intended that these should remain in all cases. In the mummy I have described, this point has been so much attended to by the embalmers, that the nail of the principal toe of the right foot having been detached, it was replaced and retained in its position by three or four turns of thread passed around it; and in this state it must have continued for the last thirty centuries.

"D. The operation of removing the cuticle being accomplished, the body was immersed into a capacious vessel, containing a liquefied mixture of wax and resin, the former predominating; and some sort of bituminous substance being added, not however essential to the process. In this situation the body was suffered to remain a certain number of days over a gentle fire, with the avowed intention of allowing the liquefied mixture to penetrate the innermost and minutest structure; nor can there exist any doubt, but that on this part of the embalming process depended not only its great preservative power, but also its various degrees of perfection. Thus, when the process was properly managed and watched, mummies, such as the one under consideration, would be produced; whereas when neglected or slovenly conducted, the mummy resulting from it would present those appearances of dryness, blackness, and brittleness, together with the carbonification of the muscles and intimate adherence of the integuments to the bones, which have been noticed by Dr. Hadley, Professor Gmelin, Blumenbach, Hunter, Dr. Baillie, Mr. Brodie, Jomard, and others, when they examined imperfect or inferior mummies. The fraudulent subtraction of the allotted quantity of wax equired for the prin-

cipal and important part of the embalming process we are now considering, or the neglecting to regulate the fire in using the wax and bitumen, would necessarily give rise to the latter results, which the covering bandages were sure to hide from the eye of the surviving relatives to whom the body was to be returned. It is also fair to presume, that inability or unwillingness on the part of friends and relatives to pay for the ingredients or for the labour necessary to carry on the operations just described, have, on many occasions, been the cause of mummies being prepared in that imperfect manner which has been noticed in so many instances.

"E. When the body was taken out of the warm liquid mixture, every part of it must have been in a very soft and supple condition, wholly unsusceptible of putrefaction. The next steps therefore to be taken with a view to convert it into a perfect mummy, must have been those which, had they been taken before that part of the process that has been just described, would have exposed the body to inevitable putrefaction, in a climate like that of Egypt. I allude to the tanning of the integuments, and the exposing of their surface to the additional influence of those salts, the presence of which, as well as that of tannin, I have most clearly demonstrated.

"Whether an infusion of the vegetable astringent employed for tanning the integuments was had recourse to in the first instance, and the immersion of the body into the concentrated water of the natron lakes followed, or whether the tanning liquid was itself made by infusing the vegetable astringents themselves in the water of the natron lakes, and the body then immersed into it, are questions, which it is neither possible nor important to decide; the body was unquestionably submitted to the operation of both those means, but in what order it is difficult to ascertain; and when the embalmers judged by the condition of the integuments, that they were sufficiently impregnated with the active principles employed, the body was allowed to dry for a few hours, and then the bandages previously prepared with a solution of *tannin* also, as proved by my experiments, were applied to the different parts, beginning with each separate limb.

"While the operation of bandaging took place, the mummy must have been in a very supple state, else the numerous deep longitudinal wrinkles observed in all those parts where the integuments are generally looser, as in the upper part of the thighs and arms, as well as over the abdomen, and at the breasts, could not have existed. These wrinkles, so well marked in the Plate, must have been produced by the bandages at the time of their application.

"It appears also, that with a view of rendering the bandages more supple in particular places, where such a condition was required, and of obviating the inconvenience of slackness in

some of the turns, they were daubed over in a few places with two different substances, the one consisting of wax and resin, the other of resin alone, both applied warm; so that, while the first served to give pliancy to some of the linen employed, the second caused the slack and loose edges of the bandages to adhere together, by which process the whole was rendered compact and firm, without producing hardness.

"The lumps of myrrh, resin, and bituminous earth, noticed in the abdomen, were pushed up through the enlarged aperture of the anus, immediately before the application of the bandages, for the purposes already detailed." E. W. B.

(To be continued.)

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Jan. 26 (*continued*).—A paper was read, On the Barometer; by J. F. Daniell, Esq. FRS.

The author, referring to some former papers which he had presented to the Society, supplies the deficiency, which he therein regretted, of evidence of the gradual deterioration of barometers, from registers which had been continued for a sufficient length of time, with the same instruments, to establish the fact. From the Transactions of the Meteorological Society of the Palatinate, he has now extracted the mean annual heights of the mercurial column at eight different stations in Europe, for twelve successive years, and dividing them into periods of six years each, he has shown that the averages of the last six are invariably lower than those of the first six. He has also made another remark, which, he observes, might have been anticipated from theory, that the amount of the depression depends, in some measure, upon the elasticity of the medium in which the instrument is placed. The five series of observations, whose mean pressure is 29·235 inches, exhibit an average depression of ·059 inch in twelve years; while the three series, whose mean pressure is 25·977 inches, present a depression of only ·026 inch in the same interval.

From the same Transactions an extract is also made of some observations of Hemmer, strongly corroborative of the opinion, that the air gains access to the *vacuum* by means of the glass and not of the mercury.

Mr. Daniell proceeds to state, that the results of the experiments which he has instituted with the platinum guard are satisfactory, as far as there has been time for their developement. In

a comparison which he has lately had an opportunity of making with some fresh-boiled barometers of Prof. Schumacher, his own guarded barometer, which has been made about fifteen months, and was formerly in perfect accordance with the Royal Society's standard, agreed perfectly with the new instruments, but stood higher than the standard; and this result was confirmed by a comparison with another barometer belonging to Mr. Newman. The author remarks that it is true the difference of ·006 inch is but small, but that the depression is as much as could have been expected, and that the whole number of observations being consistent gives weight to the conclusion.

Mr. D. next brings forward some highly curious observations of Dr. Priestley, which not only confirm his own opinions, but establish the accuracy of Mr. Faraday's observations upon the escape of gases from glass vessels, in which he had attempted to confine them by mercury, and their preservation by water, under similar circumstances, which the author remarks, has been very unnecessarily called in question. Dr. Priestley found, in his experiments upon air, that when he was operating with mercury, atmospheric air obtained admission into his jars even when there was an inch of mercury on the outside, and a column of two or three inches within; and he remarks that this is owing to there being no complete contact between mercury and glass, and that the air which is confined between the two is continually protruded forward by the vibrations of the vessel. He also observed that when a little water was placed upon the outside of the jar upon the mercury, that neither air nor water ever got in to disturb his experiments.

The paper concluded with a brief and connected view of the author's observations upon the barometer derived from the present communication, and from two former papers, which, he observes, the Council have done him the honour to place amongst their archives, from which it appears that he has established the following facts:—

1. That air gradually insinuates itself into the best made barometers of the common construction.
2. That this does not take place from any solution of the air by mercury.
3. That the passage of the air is between the mercury and the glass.
4. That the gradual deterioration of barometers may be prevented by a ring of platinum cemented to the open end of the tube.

Feb. 2.—A paper was read, On the Magnetizing Power of the more refrangible Rays of Light; by Mrs. Mary Somerville: communicated by William Somerville, MD. FRS.

In this paper, Mrs. Somerville first mentions some preceding statements on the subject: Prof. Morichini, of Rome, announced that he had succeeded in magnetizing a needle by exposing it to

the violet ray of the solar spectrum; the experiment was repeated, without success, by Prof. Configliachi at Pavia, and M. Berard, at Montpellier; Dr. Brewster states, in his Treatise on New Philosophical Instruments, that Sir H. Davy and the late Prof. Playfair witnessed a successful repetition of it in Italy; but from the indistinct and contradictory results that had been obtained even in that country, it had been concluded that the experiment was still more unlikely to succeed in our own northern climate, and no further elucidation of the subject had been obtained.

The unusual clearness of the weather, however, last summer, had induced Mrs. S. to institute some experiments on the subject; which she next proceeds to detail.

An equiangular prism of flint-glass being placed in an aperture in a window-shutter, a sewing needle, about an inch long, which had been previously ascertained to be devoid of magnetism by its attracting indifferently either pole of a magnetized needle,* was exposed to the violet ray of the spectrum, thrown on a pannel, at the distance of about five feet. One half of the needle was covered with paper, as the author did not deem it likely that polarity would ensue from the action of the light, if the whole of the needle were uniformly exposed to its influence. In about two hours, the needle became magnetized, the exposed end being found to be the north pole. The experiment having been many times repeated with the violet ray, and always with success, the blue and green rays of the spectrum were next ascertained to produce a similar effect, but in a less degree, and the indigo ray in a degree nearly as great as the violet. The yellow, orange, and red rays had no effect whatever on the needles exposed to them, even when the experiments were continued for three successive days; nor was any magnetism developed by the calorific rays, which showed that heat had no share in causing the results.

Pieces of clock and watch-springs, about an inch and a half long, and from an eighth to a quarter of an inch in width, previously ascertained to be unmagnetic, or reduced to that state by heating them, were exposed in the same manner to the more refrangible rays, and they also were rendered magnetic, the exposed ends always becoming north poles. They appeared indeed to be more susceptible of magnetization than the needles, probably on account of their greater extent of surface and blue colour. Bodkins were not affected, owing, perhaps, to their greater mass. When the violet ray was concentrated by means of the large lens employed by Dr. Wollaston in his experiments

* The magnetic needle employed for this purpose was also a sewing needle, which, after it had been magnetized in the usual way, was driven through a cork in which a glass cap was inserted; and it was then suspended, so as to revolve freely, on the point of another sewing needle.

on the chemical rays, magnetism was imparted to steel in a shorter time than by that ray in its ordinary state.

It was found to be unnecessary to darken the room for these experiments, it being sufficient to throw the spectrum on a part of the room where the sun's rays did not shine.

Mrs. Somerville next tried the effect of the solar rays as transmitted by blue glass; and on needles being exposed, half covered as before, under glass coloured blue by cobalt, care being taken that no magnetic substance was present, they also were magnetized. It was not ascertained whether the rays which produce chemical charges had any share in this effect; for by subjecting two slips of paper, dipped in solution of muriate of silver, to the action of the sun's rays under the blue and under common white glass, both were blackened in the same time and to the same degree. Needles exposed in the same manner under green glass were also magnetized.

By inclosing needles in pieces of green and blue riband, half of each being covered with paper, and hanging them up in the sun for a day, behind a window-pane, they likewise acquired polarity; the exposed ends becoming north-poles as usual. But no effect was produced, by the same treatment, on needles inclosed in red, orange, or yellow silk.

Throughout the experiments detailed in this paper, with a very few exceptions, seemingly attributable to a predisposition to magnetism too slight to be detected, the exposed end of the needle, &c. employed, became the north-pole. From ten to twelve and one o'clock appeared to be the most favourable time for the experiments. As the season advanced, the magnetism acquired was less permanent, or the needle required exposure for a longer period to render it permanent, and the effect in general decreased. The author infers from the whole, that the more refrangible rays of light have the property of imparting magnetism.

The reading was also commenced of a paper, On the Action of Sulphuric Acid upon Naphthaline; by M. Faraday, Esq. FRS.

Feb. 9.—James Holman, Esq. was admitted a Fellow of the Society; and the reading of Mr. Faraday's paper was continued.

Feb. 16.—Charles Lyell, Esq. FL. and GS. and Dr. J. A. Ogle were respectively admitted Fellows of the Society, the name of John Hawkins, Esq. ordered to be inserted in its printed lists; and the reading of Mr. Faraday's paper was concluded.

In consequence of the peculiar action of sulphuric acid upon certain hydrocarbons, observed by the author, and referred to in a late paper on New Compounds of Carbon and Hydrogen,* he was led to examine the effect produced by treating various substances with sulphuric acid, and amongst others, naphthaline. It was soon found that a new compound was produced, very peculiar in its composition and properties, the existence of

* See *Annals* for January, p. 48.

which accounted for the different statements made by various writers respecting the mutual action of the two substances.

Cold pulverized naphthaline having been put into three or four parts of cold concentrated sulphuric acid, and agitated and left for several days, was gradually dissolved, forming a red mixture of a crystalline and a fluid matter, which, upon the addition of water, dissolved almost entirely, yielding a solution of a peculiar, bitter, acid taste.

Upon fusing about two parts of naphthaline with one part of strong sulphuric acid in a flask, and agitating them together, a perfect mixture was obtained, but which on standing separated, whilst fluid, into two portions, both deep-red, but the heavier much more so than the lighter. Being poured into tubes, retained hot for some time to admit of separation, and then allowed to cool, both substances crystallized, and became opaque and deep-red. Being separated they were examined; the upper was crystalline, and hard like naphthaline; it was sapid; and being heated with a little water, was separated into nearly pure naphthaline which floated, and an acid which dissolved in the water: the lower substance was much heavier than the former, softer, crystalline, deep-red in colour; exposed to the air it became moist on the surface from the commencement of deliquescence, and then harsh from the separation of naphthaline. It was highly bitter, and burnt in the air with much flame. When rubbed with water, about a fourth part of its weight of naphthaline separated, the rest being soluble, and similar to that extracted from the lighter substance.

Upon neutralizing some of this acid with potash, and digesting the salt formed with alcohol, it was found that the alcohol upon evaporation left a white, dry, and crystalline salt, which, when heated in the air, burnt with much flame, and which, being soluble in alcohol, and not precipitating salts of barytes and lead, was strongly distinguished from a sulphate.

In this way it appeared that sulphuric acid and naphthaline might be combined producing a new acid, which, as it existed in large quantities in the heavier substance, mixed with free sulphuric acid, was now to be separated from it, and obtained in a pure state. For this purpose a specimen of native carbonate of barytes was selected, and rubbed to powder with the solution of the impure acid until it had been rendered neutral; the water was found to contain a soluble barytic salt, and upon washing the solid residue at the bottom of the mortar with more water, an additional portion of the same salt was obtained, the sulphate of barytes and excess of carbonate remaining untouched. The barytic salt in solution was now decomposed by the addition of such quantity of sulphuric acid as would precipitate all the barytes and leave no excess, so that by filtration a solution of the new acid was obtained. It was colourless and transparent, did not precipitate

salts of lead or barytes, and being carefully evaporated, gave ultimately a solid crystalline acid, deliquescing in the cold air, fusing at 212°; by a higher heat, charring and burning with much dense flame.

The salts which this acid forms with bases are all soluble in water and in alcohol; the solutions upon evaporation yield the white salt in a more or less crystalline state, generally unchangeable in the air, when decomposed by heat in tubes giving off much naphthaline and vapour, and leaving a mixture of sulphate and sulphuret. When heated in the air on platinum foil, they burn with much dense flame, almost like naphthaline.

The analytical experiments were made with the salt of barytes, it being a very constant and permanent substance, capable of being dried perfectly by a heat of 212°, and not undergoing material decomposition below 500°. The quantity of barytes in the salt was ascertained by burning a given weight in a platinum crucible, and heating the residue twice or thrice with the addition of sulphuric acid; the sulphate of barytes indicated the quantity of barytes present. The quantity of sulphuric acid present was ascertained by mixing a given weight of the salt with carbonate of barytes and oxide of copper, heating the mixture to redness in a tube, then acting on the residue by nitro-muriatic acid: collecting the quantity of sulphate of barytes thus produced, it indicated the proportion of sulphuric acid. The carbon and the hydrogen were estimated in the usual way by heating the salt with oxide of copper. By this analysis the elements of the salt approximate closely to

1 proportional of barytes	78
2 ditto sulphuric acid	80
20 ditto carbon	120
8 ditto hydrogen	8

Abstracting the barytes, the remaining elements indicate the composition of the pure acid, which thus appears to contain above three-fifths of hydrocarbon; and in this state of combination, the powers of the sulphuric acid are so far reduced or neutralized by the presence of the hydrocarbon as to have the saturating power of one proportional only, though two proportionals are present—a fact also previously observed by Mr. Hennel, in the substance called sulphovinous acid.

The name of *Sulpho-naphthalic acid* has been given to this compound, as sufficiently indicating its source and nature, without involving theoretical views.

A paper, On the Circle of Nerves which connect the Voluntary Muscles with the Brain; by Charles Bell, Esq. FRSE. communicated by the President, was also read.

Feb. 23.—A paper was read, entitled "An Account of a new Reflecting Curve; with its Application in the Construction of a

Telescope having only one Reflector;" by Abram Robertson, DD. FRS. Savilian Professor of Astronomy, Oxford.

Also a paper, On the Constitution of the Atmosphere; by John Dalton, Esq. FRS.; of which we shall probably give an account in our next number.

ASTRONOMICAL SOCIETY.

Jan. 13, 1826.—There was read a paper by Stephen Groombridge, Esq. FRS. on the co-latitude of his observatory at Blackheath, as determined from his own observations. The author first describes a simple method of bringing the transit-instrument into the meridian, by the observations of Polaris and other circumpolar stars, and then by comparisons of high and low stars. He next describes the method of ascertaining the true zenith point, and thence the elevation of the pole, by observations of circumpolar stars in zenith-distance above and below the pole, from which twice the co-latitude becomes known. Employing his own constant of refraction, he obtains from observations of 32 circumpolar stars above and below the pole 77° 3′ 55″·65 for the mean double co-latitude; thence 38° 31′ 57″·82, and 51° 28′ 2″·18 for the latitude; a result which accords with his independent observations on the solstices.

Mr. Groombridge next proceeds to deduce from this, the co-latitude of the Royal Observatory. He determines the difference of the zeniths of the two observatories at 35″·25, which applied to the latitude of the Blackheath Observatory, by addition, gives 51° 28′ 37″·43 for that of the Royal Observatory, being less than Mr. Pond makes it by more than a second. Mr. Groombridge imputes the difference to an erroneous constant of refraction. The author concludes his paper, by presenting some simple formulæ for finding the position of a transit instrument, from the observed transits of a high and low star, passing the meridian to the south of the zenith; or from the observed transit of a circumpolar star above and below the pole.

There was next read, a communication, from Sir Thomas Brisbane, dated Paramatta, 2d July, 1825. The contents were, 1st. Observations with a repeating circle for the winter solstice, 1825, extending from June 12th to July 1st inclusive. These are not yet reduced. 2dly. Observations on the inferior conjunction of Venus and the Sun, in May, 1825, with the mural circle, from May 1st to 25th inclusive. 3dly. Observations on the dip of the magnetic needle, March, 1825;—the mean of the whole was 62° 41′ 35″. 4thly. Observations on the declination of the needle in March, April, and May, 1825;—the mean of the whole is 8° 59′ 48″. Lastly. An abstract of the Meteorological Journal kept at Paramatta, from April, 1824, to April, 1825.

GEOLOGICAL SOCIETY.

Jan. 20.—A paper was read, "On the Geology of Jamaica;" by H. T. De la Beche, Esq. FRS. &c. Mr. De la Beche's observations are confined to the eastern half of Jamaica, which includes the whole range of the Blue Mountains, the highest eminences of the Island, those of Port Royal, Spanish Town, the Mocko Mountains, and other ridges of inferior elevation. These heights often include or are connected with extensive plains, the principal of which are those of Liguanea, Vere, and Lower Clarendon, Luidas Vale, and St. Thomas's. The rocks of oldest formation which presented themselves to the author within this district, he refers to the submedial or transition series. They compose the greater part of the Blue Mountain range, and consist of, 1. Graywacké, both foliated and compact, coarse and fine, presenting in short the usual variations common to this rock in Europe, and appearing on some points to pass into old red sandstone; 2. Transition limestone, apparently destitute of organic remains, compact, of a dark bluish gray colour, and traversed by veins of calcareous spar; occasionally associated with argillaceous slate, and its upper beads much intermixed with sandstones. These stratified rocks throughout the Blue Mountains generally dip towards the NE and ENE at a considerable angle; but there are frequent exceptions to this rule, and the strata are on the whole much contorted. They are occasionally associated with trap rocks, viz. syenites, greenstones, and claystone porphyry.

The author observed on one point, viz. the southern slope of St. Catherine's Hill, a series of strata which he conceives to represent the coal measures; the old red sandstone is, however, developed on a larger scale, and in more numerous localities; so that the medial or carboniferous series is certainly not wanting in Jamaica. Resting upon this appears, on many points, a porphyritic conglomerate, associated with porphyry, and occasionally with greenstone and syenite. Similar trap rocks, intermixed in the most varied manner, show themselves very extensively, composing the greater part of the St. John's Mountains, and the district bordering on the Agua Alta. One variety of porphyry met with by the author is composed of nodular concretions, separated by a soft argillaceous substance, among which strings of chalcedony are sometimes found. It is remarkable that the only instance of a similar structure which has occurred to the author is in an amygdaloidal rock, decidedly of volcanic origin, at Black Hill, on another part of the island.

These trap rocks are found generally supporting the *great white limestone formation*, which occupies a very large portion of the whole island. This formation, from the fossils it contains, is referred by Mr. De la Beche to the tertiary series. It is principally composed of white limestone, most frequently very

compact, and then strongly resembling the compact varieties of Jura limestone. The strata are usually very thick, varying from three to twenty feet in breadth. In some districts this rock is interstratified with thick beds of red marle and sandstone, and white chalky marles. The compact limestone constitutes the middle part of the formation. The lower beds consist chiefly of sands and marles, sometimes associated with bluish gray compact limestones, at others with beds of earthy yellowish white limestone, containing an abundance of organic remains, viz. *Echinites*, *Ostreæ*, and particularly large species of *Cerithium*. The upper beds of the formation are rather chalky, sandy, and marly, and contain numerous remains of the genera *Conus*, *Cerithium*, *Astarte*, *Natica*, &c. and near the sea-coast a great quantity of Corals, which, frequently, have almost a recent appearance.

Above the white limestone formation, beds of conglomerate and sandstone are visible on many points, particularly on the edges of the Savannahs; whence the author calls them the *Savannah sandstones*.

The upper beds of all visible in the island consist of *diluvium* and *alluvium*. The former shows itself on a very large scale, covering the surface of the principal plains, particularly that of Liguanea.

It consists of rounded fragments of the rocks which compose the neighbouring mountains. The Hope river, which has cut its channel through the plain of Liguanea, has exposed sections of these diluvial gravel-beds, from 200 to 300 feet in thickness. The greater part of the large plain of Vere and Clarendon is also composed of diluvium. The pebbles of these beds consist chiefly of trap rocks; those of white limestone are comparatively rare, this rock appearing to have failed in resistance to the force of attrition by which its fragments were attacked. The separation between the diluvium and alluvium is not very decided; but deposits of the latter class have certainly been produced in considerable quantities along the course of many of the rivers, and on parts of the shore, particularly between Kingston and Port Henderson, in front of which extends a long sand-bank, called the Palisades.

Mr. De la Beche's paper concludes with an interesting comparison of the Jamaica formations with those of Mexico and South America, as described by M. de Humboldt. The graywacké of Jamaica would seem to be continued in Mexico, with its accompanying trap rocks, and dark-coloured limestones. In South America it is absent, and its place is supplied solely by porphyries, syenites, and greenstones, which are developed there on a very large scale. The red sandstone which is found in Jamaica, occurs very extensively in the neighbouring parts of the American continent. A formation analogous to the white

limestone of Jamaica seems, from M. de Humboldt's description, to occur both in Mexico and Venezuela.

Feb. 3.—A paper was read, entitled, "Remarks on some Parts of the Taunus Mountains, in the Duchy of Nassau;" by Sir A. Crichton, VPGS. &c. An abstract of this paper will be given in our next.

MEDICO-BOTANICAL SOCIETY OF LONDON.

On Monday, Jan. 16, this Society held its Anniversary Meeting; when the following Officers and Council were elected for the present year:—

Council.

President.—Sir J. M'Gregor, MD. FRS.

Vice-Presidents.—W. T. Brande, Esq. FRS.; Sir A. Cooper, Bart. FRS.; Sir A. Crichton, FRS. Sir W. Franklin, FRS.; E. T. Munro, MD.; J. A. Paris, MD. FRS.

Treasurer.—H. Drummond, Esq. FSA.

Director.—J. Frost, Esq. FSA.

Auditor of Accounts.—W. Newman, Esq.

Secretary.—R. Morris, Esq. FLS.

T. Gibbs, Esq. FHS.; T. Gordon, MD. MRAS.; T. Jones, Esq.; G. H. Roe, MD.; J. G. Smith, MD.; W. Yarrell, Esq. FLS.

The gold medal of this Society was awarded to Matthew Curling Friend, Esq. Lieutenant in the Royal Navy, and FRS. for his communication respecting certain articles of the materia medica used in Africa; and the silver medal to James Hunter, Esq. FHS.

E. W. B.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Account of Prof. Berzelius's Method of detecting Arsenic in the Bodies of Persons poisoned.*

Prof. Berzelius has lately given some instructions for the discovery of arsenic in persons that have been poisoned with it. He considers the *reduction of arsenic to the metallic state as the only incontestible proof of the presence of this poison.* Arsenic may occur in two ways, viz. when it is found in substance (in the state of arsenious acid) in the dead body, and when it is not found in this state; though the intestines of the dead body may contain it in the state of a solution.

In the first of these cases, it is easy to determine the presence

of arsenic. In order to do this, take a piece about three inches long, of an ordinary barometer tube, and having drawn out one end of it into a much narrower tube close the end. Let some of the arsenic found in the body be now put in at the open or larger end, so that it may fall down to the bottom. Any quantity of this arsenic of sufficient volume to be taken from the body will suffice for this purpose. The arsenic being at the bottom of the small part of the tube, a little charcoal is let fall upon it, after it has been freed from all moisture by bringing it to a red heat with the blowpipe. The charcoal is then heated in the tube at the flame of a spirit-lamp, the point where the arsenic lies being held out of the flame. When the charcoal is very red, the point containing the arsenic is drawn into the flame. The arsenic is then instantly volatilized, and passing into vapour by the red charcoal, it is reduced, and reappears on the other side of the flame in a metallic state. The flame is then brought slowly towards the metallic sublimate, which is thus concentrated into a smaller space in the small tube; and then presents a small metallic ring shining like polished steel.* We have now only to verify, by its smell, that the metallic sublimate is arsenic. For this purpose, cut the small tube with a file a little above the sublimate, and having heated the place where it lies, put the nose above it at a small distance, and the particular odour of the metal will be immediately perceived.

In the case where the solid arsenic cannot be found, we must collect as much as possible of the contents of the stomach and the intestines, or even cut the stomach in pieces, and mix it with its contents. The whole is then to be digested with a solution of hydrate of potash. Hydrochloric acid is then added in excess. The whole is filtered, and, if the liquid is too much diluted, it is concentrated by evaporation. A current of sulphuretted hydrogen is then passed through it, which precipitates the arsenic in the form of the yellow sulphuret. If the quantity of arsenic is very small, the liquid will become yellow without giving a precipitate. It must then be evaporated, and in proportion as the hydrochloric acid becomes more concentrated, the sulphuret of arsenic will begin to be deposited. It is then filtered. If the sulphuret remaining on the filter is in too small a quantity to be taken from the paper, add some drops of caustic ammonia, which will dissolve it. Then put the liquid which passes the filter into a watch-glass, and evaporate it. The ammonia will be volatilized, and will leave as a residue the sulphuret of arsenic. If it shall still be difficult to collect the sulphuret, we must put into the watch-glass a little pulverized nitrate of potash, and, with the finger, mix the sulphuret with the nitrate

* Had the experiment been made in the wide part of the tube, the result would scarcely have been visible with a small quantity of arsenic.

of potash, which detaches it from the glass. At the bottom of a small phial, or a piece of glass tube, shut at one end, melt a little nitrate of potash at the flame of a spirit-lamp, and introduce into it, when melted, a little of the mixture which contains the sulphuret of arsenic. It is oxidized with effervescence, but without fire, or detonation, and without loss of arsenic. The melted salt is then to be dissolved in water, and lime added in excess, and the liquid boiled. The arseniate of lime will then be deposited, and may be collected. When dried it is mixed with charcoal, and then brought to a red heat by the blowpipe; and a small quantity of this mixture is allowed to fall to the small end of the above-mentioned tube. It is now gradually heated to expel all humidity which tends to throw it into the wide part of the tube, and when it is very dry, heat, at the flame of the blowpipe, the part of the tube which contains the mixture. The arsenic will be disengaged, and be sublimed at a distance from the heated part. An addition of vitrified boracic acid greatly promotes the decomposition which then takes place at a less elevated temperature; but the acid frequently contains water, and produces a bubbling of the melted matter, which raises it in the tube, and causes the vapours to issue by perforating the softened part of the glass.

M. Berzelius maintains, that the *sixth part of a grain of sulphuret of arsenic is sufficient to make three different trials;* but he adds, that when we have discovered only very small traces of arsenic, we must take care not to introduce any by means of re-agents, among which, both the sulphuric and the hydrochloric acid may contain it. The first almost always contains some arsenic when it is manufactured from volcanic sulphur, and the second in consequence of sulphuric acid being used in the preparation of hydrochloric acid, yields the arsenic which it contains in separating it from soda. We must, therefore, be certain of the purity of these re-agents.

When death has been caused by the arsenic, and not by the arsenious acid, the process must be modified, because the sulphuretted hydrogen gas decomposes the arsenic acid too slowly. In this case, we must add hydrosulphuret of ammonia, which reduces the arsenic acid to the state of sulphuret, which is afterwards precipitated by the hydrochloric acid.*—(Edin. Phil. Jour.)

* It is obvious that Berzelius has not seen Dr. Christison's paper on the "Detection of minute Quantities of Arsenic in mixed Fluids." These gentlemen agree in precipitating arsenious acid by sulphuretted hydrogen, so as to obtain the yellow sulphuret; but their subsequent methods differ. Berzelius adopts a process which requires all the dexterity of as expert a chemist as himself for conducting it with success. Dr. Christison, on the contrary, scrapes the sulphuret from the filtre with a knife, which may be done though a very minute portion of it is present, and obtains metallic arsenic at once by heating it with black flux. We refer for particulars to his paper in the Edinburgh and Surgical Journal.—(*Note by the Editor.*)

Mineralogy.

2. *Professor Berzelius's Researches on Molybdæna.*

In studying the properties of molybdæna, M. Berzelius has found that this metal, of which we knew only the purple oxide, produced by drying the blue oxide, and molybdic acid, has two salifiable oxides, whose saline combinations were till now unknown. The deutoxide may be procured by digesting a mixture of molybdic acid, metallic molybdæna, and sulphuric or hydrochloric acid, till the colour of the liquid becomes a deep red. Instead of metallic molybdæna, we may substitute metallic copper. The red liquid gives, with ammonia, a rust-yellow precipitate, which is the hydrate of the deutoxide of molybdæna. This hydrate is very soluble in water. When it is washed, the water, after having removed the saline substances which caused its precipitation, begins to dissolve the hydrate, and becomes yellow. It at last dissolves it entirely, and the saturated solution is red. It reddens turnsol. The hydrate dissolves in acids, and gives salts whose solutions are red, but which, when evaporated to dryness, are almost black.

The protoxide is produced when we macerate the solution of a salt with a base of the deutoxide, with mercury, and add, from time to time, a liquid amalgam of potassium. The colour of the liquid becomes deeper, and ends by growing black. Before the introduction of the amalgam, we must add to it hydrochloric acid, in order to prevent a part of the deutoxide from being precipitated before its entire reduction to the protoxide. The black solution is then precipitated by ammonia, and the black precipitate is the hydrate of the protoxide, which must be well washed, and then dried in vacuo. The hydrate appears then under the form of a jet black powder. When heated in vacuo it gives out slowly its water, and afterwards, at a temperature which approaches to that of brown-red, it takes fire, and burns with scintillation. The barometer of the air pump is not affected by this phenomenon, which, in other respects, is of the same nature as that which is observed in the hydrate of the peroxide of iron, and the protoxide of chrome and of zircon. The anhydrous protoxide is insoluble in acids; when heated in air it takes fire, and burns feebly, producing the brown oxide of molybdæna. The salts of this oxide are black, and their dilute solutions have a compound colour of green, black, and brown, though sometimes they assume a fine purple colour. The fluate of the protoxide, for example, is a very fine purple, and the double fluates with potash, soda, and ammonia, are of a rose red colour.

In order to form the protoxide of molybdæna, we may make use of zinc in the place of the amalgam of potassium; but the protoxide then retains the oxide of zinc in a very obstinate manner.

What is called molybdous acid, that is to say, the blue oxide of molybdæna, is not a particular acid. It cannot be combined with alkalies, which, on the contrary, decompose it, by precipitating the hydrate of the yellow oxide, and combining with the molybdic acid. It may be produced most readily in dissolving the bimolybdate of ammonia, and adding to it a solution of a salt with a base of the deutoxide. It produces a precipitate of a fine deep blue, which is very soluble in water, and is only deposited because the water contains salts. We may wash it with a solution of sal ammoniac, afterwards removing the salt by a little cold water. It gives with warm water a blue solution, highly saturated, which may be easily preserved at the ordinary temperature of the atmosphere. In the dry form it resembles indigo, and retains its solubility in water.

Prof. Berzelius has found, that the deutoxide of molybdæna is composed of one atom of molybdæna and two atoms of oxygen. The molybdic acid contains three atoms. The blue oxide is a bymolybdate of the deutoxide of molybdæna, that is, $\ddot{\mathrm{Mo}}$. + 4 $\dddot{\mathrm{Mo}}$. There is still another combination between the oxide and the acid which is produced when the blue liquid is digested with metallic molybdæna. It is green, equally soluble in water, and precipitable in sal ammoniac. M. Berzelius supposes its composition to be $\ddot{\mathrm{Mo}}$. + 2 $\dddot{\mathrm{Mo}}$. Tungstic acid likewise combines with the deutoxide of molybdæna, and the combination is very soluble in water, and of a superb purple colour. It is also precipitated by sal ammoniac.

The molybdic acid performs the part of a base towards the stronger acids. M. Berzelius has examined them in this point of view, and has described some of the salts which it forms.

M. Berzelius has discovered a new sulphuret of molybdæna, proportional to the molybdic acid. It is of a ruby colour, transparent, and crystallized. It combines with the metallic protosulphurets, and forms with them particular salts, of which a great number are soluble in water.

Molybdæna combines with chlorine in three proportions. The first is red, and a little volatile. The second is black, very fusible, very volatile, and crystallizes in a black mass, of a brilliant colour, like iodine, which it resembles even in the colour of its gas, which, however, is more red than violet. The third is colourless, and crystallizes in scales. These three chlorides correspond to the muriates of the protoxide, of the deutoxide, and of the peroxide, that is to say, of the acid. Iodine does not combine in the dry way with molybdæna, but the hydriodic acid dissolves the protoxide and the deutoxide. The molybdic acid decomposes it, and separates the iodine from it.

The best method of obtaining molybdæna in some quantity is, to heat the molybdic acid in a porcelain tube. When this tube is heated to redness, there is introduced into it a current of hydrogen gas, which is continued as long as it produces water.—(Edin. Journ. Science.)

Miscellaneous.

3. *On Native Silver from Michigan.* By H. R. Schoolcraft.

Mineralogical and Chemical Characters.—By examining this mineral, it will be perceived to possess the colour, lustre, malleability, and other obvious characters of native silver. It is so soft, as to be easily cut by the knife; and in a state of purity which permits it to spread under the hammer. These characters serve to distinguish it from antimonial silver, which is not *malleable;* from native antimony, which tarnishes on exposure, &c. The metal occurs in thin massive veins in the rock. These veins sometimes intersect, but never cross each other. It is also disseminated in small particles through the stone, or spread in flattened masses over its surface. Some of these masses were detached by the discoverer, but have been preserved, and are presented to the Lyceum with the more solid and undisturbed portions.

By submitting a small portion of the metal to the action of nitric acid, I obtained an imperfect solution. On repeating the experiment, and adding a little sulphuric acid, the action was more brisk, and a clear and apparently perfect solution effected. By standing, however, a pulpy white precipitate appeared at the bottom of the glass. This was collected, and submitted to the action of the blowpipe, on a basis of charcoal. The result gave a number of minute, metallic globules, possessing greater lustre, malleability, and ductility, than the original mass. I repeated the latter experiment, adding to the nitro-sulphuric solution muriate of soda. A more perfect precipitation of the white powder was effected; but the results with the blowpipe remained the same.

Geognostic Position.—It is a rolled mass. An opinion of the specific character of the rock may be dubious, from the smallness of the specimen. It appears to have been detached from a stratum of gneiss, and is essentially composed of quartz. The blackish colour of some parts of this latter mineral would, at a first glance, lead us to attribute this colour to the presence of hornblende; but on a closer examination, it will be perceived to be owing to a dark coloured steatite, which, in certain parts of the rock, is well developed, soft, and easily cut. A little calcspar is intermingled with the steatite.

Locality.—I am indebted to the politeness of Lieut. Lewis S. Johnston, of the British Indian Department, at Malden, (U. C.)

for the opportunity of adding this specimen to the mineralogical cabinet of the Lyceum. This gentleman, as he informed me, obtained it from the south-eastern shores of Lake Huron, near Point aux Barques, in Michigan Territory. That part of Lake Huron was cursorily examined by me, in the year 1820, in the course of the expedition conducted by Gov. Cass, through the upper lakes, &c. We considered it remarkable, even in a region abounding in rolled rocks, for the great number and variety of granite, gneiss, hornblende, and trap bowlders, scattered along the shores of the lake. The water here is generally shallow, and dangerous to approach in vessels; these bowlder stones sometimes extending and presenting themselves above water for a mile or more from land. But we could not satisfy ourselves, by an examination necessarily partial, that either of the primitive species mentioned, existed there in any other condition than as rolled masses, or displacements of rock strata, contiguous, perhaps, but not observed. Dr. Bigsby has informed me, that he observed the gneiss, *in situ*, on the north-western shores of this lake. The nearest rock in place, and that which in fact constitutes the abraded and caverned promontory of Point aux Barques, is gray sandstone.—(Annals of the Lyceum of Natural History of New York.)

4. *Method of browning Iron.* By Mr. J. Duntze, of New-Haven.

Nitric acid	$\frac{1}{2}$ ounce.
Sweet spirits of nitre	$\frac{1}{2}$ ditto.
Spirits of wine	1 ditto.
Blue vitriol	2 ditto.
Tincture of steel	1 ditto.

These ingredients are to be mixed, the vitriol having been previously dissolved in a sufficient quantity of water to make, with the other ingredients, one quart of mixture. Previously to commencing the operation of browning a gun-barrel, it is necessary that it be well cleaned from all greasiness and other impurities, and that a plug of wood be put into the muzzle, and the vent well stopped. The mixture is then to be applied with a clean sponge, or rag, taking care that every part of the barrel be covered with the mixture, which must then be exposed to the air for twenty-four hours, after which exposure the barrel must be rubbed with a hard brush, to remove the oxide from the surface.

This operation must be performed a second and a third time (if requisite), by which the barrel will be made of a perfectly brown colour. It must then be carefully brushed and wiped, and immersed in boiling water, in which a quantity of alkaline matter has been put, in order that the action of the acid upon

the barrel may be destroyed, and the impregnation of the water by the acid neutralized.

The barrel, when taken from the water, must, after being rendered perfectly dry, be rubbed smooth with a burnisher of hard wood, and then heated to about the temperature of boiling water; it then will be ready to receive a varnish made of the following materials :—

Spirits of wine, one quart,
Dragon's blood pulverized, three drams,
Shell lac bruised, one ounce;

and after the varnish is perfectly dry upon the barrel, it must be rubbed with the burnisher to give it a smooth and glossy appearance.—(Silliman's Journal.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Dr. J. Mason Good is preparing for publication a new work, to be entitled "The Book of Nature," being a Series of Lectures formerly delivered at the Surrey Institution, as a popular Illustration of the general Laws and Phænomena of the Creation. It will be in three volumes, 8vo.

JUST PUBLISHED.

Shaw's General Zoology, or Systematic Natural History, continued by James F. Stephens, FLS. &c. Vol. 13. In Two Parts.

An Essay on the Application of Lunar Caustic, in the Cure of certain Wounds and Ulcers. By John Higginbottom, Mem. Roy. Coll. Surg. London. 8vo. 6*s*. 6*d*.

Numerous Cases illustrative of the Efficacy of the Hydrocyanic or Prussic Acid in Affections of the Stomach. By John Elliotson, MD. 8vo. 5*s*. 6*d*.

Dewce on the Medical Treatment of Children.

Forsyth's New Medical and Surgical Dictionary. 8vo. 15*s*.

Article XV.

NEW PATENTS.

R. Stevenson, Bridge Town, Warwickshire, engineer, for axletrees to remedy the extra friction on curves to waggons, carts, and carriages, used on rail roads, trainways, and other public roads.—Jan. 23.

R. Rigg, Bowstead Hall, Cumberland, for a new condensing apparatus to be used with the apparatus now in use for making vinegar.—Feb. 4.

J. C. Gamble, Dublin, chemist, for an apparatus for the concentration and crystallization of aluminous and other saline and crystallizable solutions, part of which apparatus may be applied to the general purposes of evaporation, distillation, inspissation, and desiccation, and especially to the generation of steam.—Feb. 7.

W. Mayhew, Union-street, Southwark, and W. White, Cheapside, hat manufacturers, for an improvement in the manufacture of hats.—Feb. 7.

H. Evans, harbour-master of the port of Holyhead, North Wales, for a method of rendering ships and other vessels, whether sailing or propelled by steam, more safe in cases of danger by leakage, bilging, or letting in water, than as at present constructed.—Feb. 7.

W. Chapman, Newcastle-upon-Tyne, civil engineer, for improved machinery for loading or unloading of ships, vessels, or craft.——Feb. 7.

B. Cook, Birmingham, brass-founder, for improvements in making files of various descriptions.—Feb. 7.

W. Warren, Crown-street, Finsbury-square, for improvements in the process of extracting from the peruvian bark, medicinal substances or properties, known by the name of quinine and cinctronine, and preparing the various salts to which these substances may serve as a basis.—Feb. 11.

J. L. Higgins, Oxford-street, for improvements in the construction of the masts, yards, sails, rigging of ships, and smaller vessels, and in the tackle used for working or navigating the same.—Feb. 11.

B. Newmarch, Cheltenham, and C. Bonner, Gloucester, brazier, for a mechanical invention to be applied for the purpose of suspending and securing windows, gates, doors, shutters, blinds, and other apparatus.—Feb. 18.

T. Walter, Luton, Bedfordshire, straw-hat manufacturer, for improvements in the manufacture of straw plat, for making bonnets, hats, and other articles.—Feb. 18.

C. Whitlaw, Bayswater Terrace, Paddington, medical botanist, for improvements in administering medicines by the agency of steam or vapour.—Feb. 18.

A. Buffum, Bridge-street, hat manufacturer, for improvements in the process of making or manufacturing and dyeing hats.—Feb. 18.

ANNALS

OF

PHILOSOPHY.

APRIL, 18

Article I.

On Solutions of the Function $\psi^z x$, *and their Limitations.* By Mr. W. G. Horner.

(*Concluded from* p. 173.)

9. From the preceding train of argument, in which I hope I have erred, if at all, only in preferring minuteness to obscurity, it will be evident, among other things, that I was correct in excluding $k = \frac{n}{2}$ from the solution

$$\psi x = \varphi^{-1}\left\{\frac{a + b\,\varphi\,x}{c - \frac{b^2 - 2\cos.2\,\vartheta\,b\,c + c^2}{(2 + 2\cos.2\,\vartheta)\,a}\,\varphi\,x}\right\} \ldots\ldots\ldots (28)^*$$

and that *two perfectly distinct genera* must be recognised, in the solution of periodic equations. This is a circumstance which has not yet, as it appears to me, attracted the attention of mathematicians, in any degree proportioned to its importance; and, nevertheless, there are few instances of the practical application of functional principles which do not exhibit strong indications of its influence. In the majority of examples, the solution of the ultimate differential equation has been only effected by making $\psi\,x$ constant; and I am not aware that any exists which has admitted of solution on the principles $\psi^2\,x = x$, and $\psi^{(>2)}\,x = x$, at the same time; or that a solution by means of a function of any one order above the second exists, which does not hold true for any other of the superior orders.

For two very instructive examples in illustration of these points, I refer the reader to Questions 408 and 409 of the Mathematical Repository, proposed and solved by Mr. Herschel.

In the former of those questions, however, Mr. H. seems to express a belief that the solution $\frac{a + b\,x}{c + d\,x}$, as well as all others, is contained in the formula

$$\psi\,x = \varphi^{-1}\left\{(1)^{\frac{1}{n}}\,.\,\varphi\,x\right\}$$

* In all the formulæ distinguished by an asterisk, ϑ is to be understood as $= \frac{k\,\pi}{n}$ restricted as in Art. 8.

"though," adds he, "to demonstrate this in any case but when $n = 2$, seems a matter of some difficulty."

I submit, that the converse of this, perhaps, is likely to be correct, viz. that all *real* solutions of $\psi^n x = x$ are comprised in the formula

$$\psi x = \varphi^{-1} \frac{a + b \varphi x}{c \mp d \varphi x}$$

which, as will readily appear, resolves itself into two distinct and very simple formulæ, according as n is $= 2$ or > 2.

In fact, by making a, c, d, vanish, and putting $b = (1)^{\frac{1}{n}}$, Mr. Herschel's formula is immediately obtained; but as it does not appear to me that this can be reduced to a real form when $n > 2$, and it has moreover, in this instance, been obtained by sacrificing the arbitrary character of the constants, we will pass on to another mode.

10. Not to multiply symbols in an easy inquiry, the reader will recollect that the values of φ and ψ change, from step to step of the process.

Make then $\varphi x = x \mp \frac{b}{d}$, and we have

$$\psi x = \varphi^{-1} \frac{\mp b c + a d}{d(b + c \mp d \varphi x)} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (29)$$

Where (1st) if $b + c = 0$, we have, putting $\frac{c^2 \mp a d}{d^2} = C$,

$$\psi x = \varphi^{-1} \frac{C}{\varphi x},$$

which is obviously a solution of $\psi^2 x = x$. But again, put $\varphi x = \pm x \surd C$, and we have

$$\psi x = \varphi^{-1} \frac{\sqrt{1}}{\varphi x} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (30)$$

for a general solution of the second order of functional circles.

But secondly, retaining b and c arbitrary in eq. 29, make $\varphi x = \frac{x}{d}$, and it becomes, putting $\pm b c + a d = B$, and $b + c = A$,

$$\psi x = \varphi^{-1} \frac{B}{A \mp \varphi x} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (31)$$

Or, putting again $\varphi x = x \surd B$,

$$\psi x = \varphi^{-1} \frac{1}{\frac{A}{\surd B} \mp \varphi x} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (32)$$

If φx be again made $= \frac{1}{x}$, we have

$$\psi x = \varphi^{-1} \left\{ \frac{A}{\surd B} \mp \frac{1}{\varphi x} \right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (33)$$

Equation (32) gives, putting $\frac{A}{\sqrt{B}} = s$

$$\psi^z x = \varphi^{-1} \left\{ \frac{1}{s} \mp \frac{1}{s} \mp \dots \frac{1}{s \mp \varphi x} \right. \dots\dots\dots (34)$$

the fraction extending to z terms. Now if φx be neglected in the last term, the law of the numerators in the converging series is well known to be

$$N_z = s\, N_{z-1} - N_{z-2}$$

and the same formula serves for the denominators also. Wherefore the roots of

$$y^2 - s\, y + 1 = 0$$

being R and r, we have

$$N_z = \alpha\, R^z + \beta\, r^z$$
$$D_z = \alpha_1\, R^z + \beta_1\, r^z$$

and the values of the constants being determined from the known values of the fractions when $z = 0$ and $z = -1$, viz. $\frac{0}{1}$ and $\frac{1}{0}$, the general value of the fraction is readily found to be

$$\frac{N_z}{D_z} = \frac{R^z - r^z}{R^{z+1} - r^{z+1}}$$

Whence, restoring φx to its place,

$$\psi^z x = \varphi^{-1} \frac{(R^z - r^z) \mp (R^{z-1} - r^{z-1})\, \phi\, x}{(R^{z+1} - r^{z+1}) \mp (R^z - r^z)\, \varphi\, x} \dots\dots (34)$$

These formulæ accommodate themselves to all conditions, by making s, that is, $\frac{A}{\sqrt{B}}$, $= 2 \cot. \vartheta$, when the lower sign appears, or $= 2 \operatorname{cosec.} \vartheta$, or $2 \cos. \vartheta$ with the upper sign; the values of R, r, being in the first case $\cot. \frac{1}{2} \vartheta$ and $- \tan \frac{1}{2} \vartheta$; in the second, $\cot \frac{1}{2} \vartheta$ and $\tan. \frac{1}{2} \vartheta$; and in the third, $\cos. \vartheta \pm \sin. \vartheta \sqrt{-1}$. The last alone appertains to periodical functions. Hence the solution of $\psi^n x = x$, is

$$\psi^z x = \varphi^{-1} \left\{ \frac{\sin. z\, \vartheta - \sin. (z-1)\, \vartheta\, .\, \phi\, x}{\sin. (z+1)\, \vartheta - \sin. z\, \vartheta\, .\, \phi\, x} \right\} \dots\dots (35)^*$$

which is reducible to

$$\psi^z x = \varphi^{-1} \left\{ \frac{1 - \frac{\sin. (z-1)\, \vartheta}{\sin. z\, \vartheta}\, .\, \phi\, x}{2 \cos. \vartheta - \frac{\sin. (z-1)\, \vartheta}{\sin. z\, \vartheta} - \phi\, x} \right\} \dots (36)^*$$

which, when $z = 1$, gives

$$\psi\, x = \varphi^{-1} \left(2 \cos. \frac{k\, \pi}{n} - \varphi\, x\right)^{-1} \dots\dots\dots\dots (37)$$

This solution, while, through the efficacy of the arbitrary function, it possesses all the generality of formula (28), has the great advantage of being convenient for differential purposes. All the partial solutions of functional equations which I have ever met with are contained in it; and being quite as simple in

its form as any of them, it may be applied to complete the general solution of many equations which have hitherto been solved only in particular cases. For instance, the general value of

$$\int \frac{dx}{x - \psi x}$$

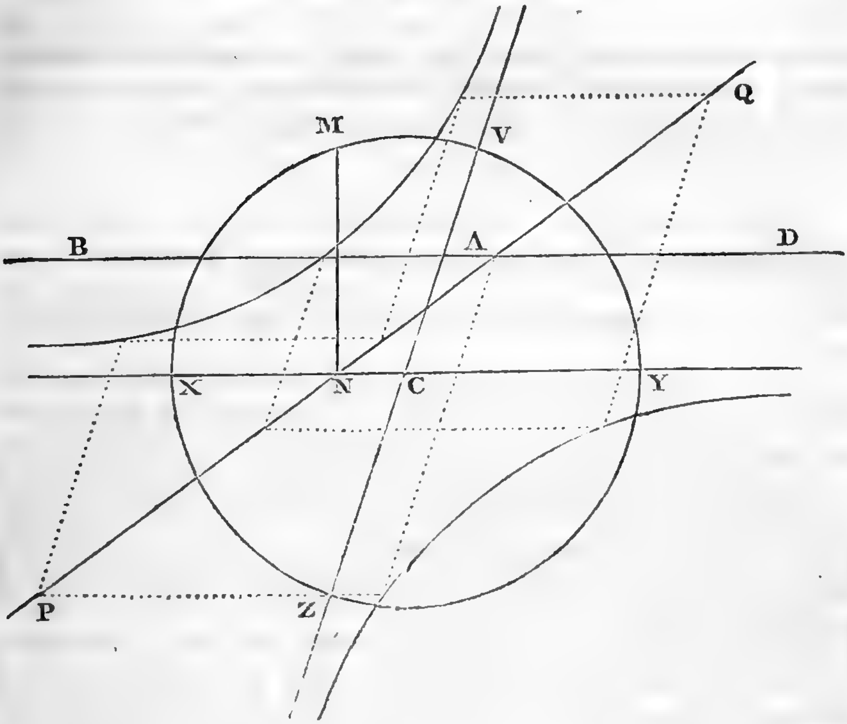
in Mr. Herschell's question, already alluded to, becomes

$$\log. \sqrt{(x^2 - 2\cos.\vartheta\, x + 1)} - \cot.\vartheta \times \tan.^{-1} \frac{x - \cos.\vartheta}{\sin.\vartheta}.*$$

11. The subject of continued fractions, which has incidentally occurred in this investigation, being intimately connected with the theory of circulating functions, and affording in consequence another proof of the existence of a sunk fence, so to say, which separates the first and second from the higher orders of functional equations, it was my intention to have pursued it in this place. The summation of a finite number of terms of a continued periodical fraction is effected in the Exercises in D. and I. Calculus, only for the two inferior orders, and is not lucidly as to the result, nor even completely, stated in La Grange's Res. des Eqq. Numeriques. But finding that my investigation and remarks would cause too long a digression, I reserve them for a future but early opportunity.

12. Geometry supplies an elegant, and, at the same time, a very simple illustration both of the nature of periodical functions, and of the restriction they are subject to. Setting aside the effect of ϕ, all our equations are manifestly those of an hyperbola taken between the asymptotes, the excentricity, in equation (32) and the following, being $= 2$, and the origin of abscissas being at the distance $\frac{A}{\sqrt{B}}$ from the centre. Now in our trigonometrical solutions, this distance is marked as cot. ϑ, or cosec. ϑ, or cos. ϑ, to radius 2. Hence, on the conditions of circulating, we have the following construction, regarding ϕ merely as a mark of an arbitrary addend effecting a proportional and simultaneous transfer both of the line and origin of abscissæ.

With centre C, the centre of a pair of hyperbolas, and radius C X = the excentricity, describe a circle intersecting one of the asymptotes in X, Y, and the other in V, Z. In the semicircle X V Y take any point M which divides it commensurately [say X M : X V Y :: m : n], and drop the perpendicular M N on X Y. Through N draw the indefinite right line O Q parallel to the common chord of the circle, and of one of the hyperbolas. Through any point A in P Q draw the indefinite right line B D, parallel to X Y, either of the asymptotes. If B D be taken as a line of successive abscissæ originating at A, and each equal and similarly affected to the preceding ordinate, then after n operations the series will recommence, and so on *in infinitum*.



In the figure $\frac{m}{n} = \frac{2}{5}$, and only those portions of the ordinates are drawn which terminate in P Q; and being connected by portions of lines parallel to the abscissæ, the whole forms an irregular polygon of n equal angles, and 2 n sides equal by pairs, which is but another effect of ϕ, as deducting equal portions from every ordinate and abscissa.

When N falls upon X or Y, this construction fails; for X V being a tangent to the hyperbola, wherever the abscissæ commence, the ordinates will continually approach the point of contact, but never pass it. This corresponds to the case $k = n$.

It equally fails when N falls upon C. For the asymptotes being properly neither abscissæ nor ordinates, and no others, in our solution, being connected with the point C, this is not a correct origin of abscissæ, so that no transfer of such origin can be made. Accordingly the only real lines of abscissæ are those which pass through the vertex of one of the hyperbolas, and the origin is at their intersection with the conjugate diameter, agreeably to equation (30), into which no circular function enters. Hence the ordinate and its abscissa are always of the same affection, both positive or both negative, which never happens in the general construction, nor in equation (37); one or more changes of sign being necessarily engaged in completing the circle of operations. This exception attaches to $k = \frac{1}{2} n$.

17. Little needs to be said on the inferior sources of perplexity, which have been imagined. It will have occurred to the reader, even in Art. 1, that $f^z x$ having two distinct meanings may be subject to limitations in one point of view, which in another have no existence. As a function of z it leaves z perfectly arbitrary, but as a function of x, and especially a periodic function, it confines our attention to the *ordinal* character of z. The same applies, perhaps, with still greater force to n, which in strictness can be no other than a term, either affirmative or negative, in the natural series of integers, agreeably to Mr. Herschell's definition (Examples, vol. ii. sect. 11). If rational fractions, as $\frac{n}{m}$, have appeared admissible, it was solely in virtue of the ordinal character of the numerator. In fact, in representing $\psi^{\frac{n}{m}} x = x$ to be a periodic formula, we make two statements, viz. $\psi x = \chi^m x$ and $\chi^n x = x$. Consequently $\psi^{\frac{-r}{m}} x = \chi^{-r} x = \chi^{n-r} x$. And this is, in every respect, the preferable mode of operating, as Mr. Herapath will find, if he applies it to the case $\psi^{-\frac{1}{4}} x$, when $\psi^{\frac{3}{4}} x = x$, which he has solved. (*Annals*, Nov. 1824.)

By reducing an irrational index to converging fractions, we may render the function susceptible of indefinite approach toward the periodical state; e. g. $f^{\sqrt{2}} x = x$, is nearly solved by $f = \chi^2$, when $\chi^3 x = x$; more nearly by $f = \chi^5$ when $\chi^7 x = x$; still more nearly by $f = \chi^{12}$ when $\chi^{17} x = x$; and so on. But such functions cannot with accuracy be termed periodical.

An imaginary index destroys the conditions of circulation, being incapable of an ordinal character.

Some caution would even appear to be requisite in interpolating even at *regular* intervals, or the conditions of the problem may be completely altered; as, for example, in taking $f^{\frac{n}{z}} x$ in Equation (26) where a given effect is to recur only at definite intervals.

W. G. Horner.

Article II.

On Mr. Horner's Solution of $\psi^z x = x$. By J. Herapath, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN, *Cranford, March* 1, 1826.

You will, I have no doubt, allow me to correct a misrepresentation in your present Number of a part of my writings printed in the *Annals* for Nov. 1824. Mr. Horner, in your Number for

March, page 173, says, "The fallacy of Mr. Herapath's attempt to prove $k = \frac{1}{2} n$ to be admissible, lies in his erroneous assumption of the 'mutual independence' of the *numerator* and *denominator* in the value of d in the present case," that is, of

$$\frac{(b + c)^2}{2\,a\,(\cos.\,\pi + 1)}.$$

Now in the above-cited Number, p. 328, my words are, using δ for the sign of differentiation, "because $\delta\,c$ and $\delta\,\pi$ are mutually independent, this value

$$\frac{\delta\,c^2}{a\,\delta\,\pi^2}$$

may be any thing." It is, therefore, the *differentials of the functional roots* of the numerator and denominator which I have called independent, and *not*, as Mr. Horner attributes to me, the *numerator* and *denominator themselves.* I have thought it necessary to notice this, because Mr. Horner has neither quoted the passage he criticises, nor even once alluded to the place or work where it is to be found. Had he done either, I should have left the simple evidence itself of what I have advanced to answer Mr. Horner's positive and groundiess charge of "fallacy" and error. I am, Gentlemen, your obedient humble servant,

J. Herapath.

Article III.

Astronomical Observations, 1826.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Occultation by the Moon.

Feb. 17. Immersion of a small star .... 7^h 27′ 30·8″ Sidereal Time.

Eclipses of Jupiter's satellites.

Feb. 26.	Immersion of Jupiter's second satellite	8^h	06′	44″	Mean Time at Bushey.
		8	08	05	Mean Time at Greenwich.
March 3.	Immersion of Jupiter's third satellite	10	34	15	Mean Time at Bushey.
		10	35	36	Mean Time at Greenwich.

Article IV.

Further Researches on the Preservation of Metals by Electro-chemical Means. By Sir Humphry Davy, Bart. Pres. RS.*

In two papers read before the Royal Society, I have described the effects of small quantities of electro-positive metals in preventing the corrosion or chemical changes of copper exposed to sea water, and I have stated that the results appear to be of the same kind, whether the experiments are made upon a minute scale, and in confined portions of water, or on large masses, and in the ocean.

The first and preliminary experiments proved, that the copper sheeting of ships might be preserved by this method; but another and a no less important circumstance was to be attended to,—how far the cleanness of the bottom, or its freedom from the adhesion of weeds or shell fish, would be influenced by this preservation.

The use of the copper sheathing on the bottom of ships is two-fold; first, to protect the wood from destruction by worms; and secondly, to prevent the adhesion of weeds, barnacles, and other shell fish. No worms can penetrate the wood as long as the surface of the copper remains perfect; but when copper has been applied to the bottom of a ship for a certain time, a green coating or rust, consisting of oxide, submuriate and carbonate of copper and carbonate of magnesia forms upon it, to which weeds and shell fish adhere.

As long as the whole surface of the copper changes or corrodes, no such adhesions can occur; but when this green rust has partially formed, the copper below is protected by it, and there is an unequal action produced, the electrical effect of the oxide, submuriate, and carbonate of copper formed, being to produce a more rapid corrosion of the parts still exposed to sea water; so that the sheets are often found perforated with holes in one part, after being used five or six years, and comparatively sound in other parts.

There is nothing in the poisonous nature of the metal which prevents these adhesions. It is *the solution* by which they are *prevented—the wear* of surface. Weeds and shell fish readily adhere to the poisonous salts of lead which form upon the lead protecting the fore part of the keel; and to the copper, in any chemical combination in which it is insoluble.

In general in ships in the navy, the first effect of the adhesion of weeds is perceived upon the heads of the mixed metal nails, which consist of copper alloyed by a small quantity of tin. The

* From the Philosophical Transactions for 1825, Part II.

oxides of tin and copper which form upon the head of the nail and in the space round it, defend the metal from the action of sea water; and being negative with respect to it, a stronger corroding effect is produced in its immediate vicinity, so that the copper is often worn into deep and irregular cavities in these parts.

When copper is unequally worn, likewise, in harbours or seas where the water is loaded with mud or mechanical deposits, this mud or these deposits rest in the rough parts or depressions in the copper, and in the parts where the different sheets join, and afford a soil or bed in which sea weeds can fix their roots, and to which zoophytes and shell fish can adhere.

As far as my experiments have gone, small quantities of other metals, such as iron, tin, zinc, or arsenic, in alloy in copper, have appeared to promote the formation of an insoluble compound on the surface; and consequently there is much reason to believe must be favourable to the adhesion of weeds and insects.

I have referred in my last paper to the circumstance of the carbonate of lime and magnesia forming upon sheets of copper, protected by a quantity of iron above $\frac{1}{120}$ parts, when these sheets were in harbour and at rest.

The various experiments that I have caused to be made at Portsmouth, show all the circumstances of this kind of action, and I have likewise elucidated them by experiments made on a smaller scale, and in limited quantities of water. It appears from these experiments, that sheets of copper at rest in sea water, always increase in weight from the deposition of the alkaline and earthy substances, when defended by a quantity of cast iron under $\frac{1}{150}$ of their surface, and if in a limited or confined quantity of water, when the proportion of the defending metal is under $\frac{1}{4000}$. With quantities below these respectively proportional for the sea, and limited quantities of water, the copper corrodes; at first it slightly increases in weight, and then slowly loses weight. Thus a sheet of copper, four feet long, 14 inches wide, and weighing 9 lb. 6 oz. protected by $\frac{1}{100}$ of its surface of cast iron, gained in ten weeks and five days, 12 drachms, and was coated over with carbonate of lime and magnesia: a sheet of copper of the same size protected by $\frac{1}{150}$, gained only one drachm in the same time, and a part of it was green from the adhering salts of copper; whilst an unprotected sheet of the same class, both as to size and weight, and exposed for the same time, and as nearly as possible under the same circumstances, had lost 14 drachms; but experiments of this kind, though they agree when carried on under precisely similar circumstances, must of necessity be very irregular in their results, when made in different seas and situations, being influenced by the degree of saltness, and the nature of the impregnations of the water, the strength of tide and of the waves, the temperature, &c.

In examining sheets which had been defended by small quantities of iron in proportions under $\frac{1}{250}$ and above $\frac{1}{1000}$, whether they were exposed alone, or on the sides of boats, there seemed to me no adhesions of confervæ, except in cases where the oxide of iron covered the copper immediately round the protectors; and even in these instances such adhesions were extremely trifling, and might be considered rather as the vegetations caught by the rough surface of the oxide of iron, than as actually growing upon it.

Till the month of July, 1824, all the experiments had been tried in harbour, and in comparatively still water; and though it could hardly be doubted, that the same principles would prevail in cases where ships were in motion, and on the ocean; yet still it was desirable to determine this by direct experiment; and I took the opportunity of an expedition intended to ascertain some points of longitude in the north seas, and which afforded me the use of a steam boat, to make these researches. Sheets of copper carefully weighed, and with different quantities of protecting metal, and some unprotected, were exposed upon canvass so as to be electrically insulated upon the bow of the steam boat; and were weighed and examined at different periods, after being exposed in the north seas to the action of the water during the most rapid motion of the vessel. Very rough weather interfered with some of these experiments, and many of the sheets were lost, and the protectors of others were washed away; but the general results were as satisfactory as if the whole series of the arrangements had been complete. It was found that undefended sheets of copper of a foot square lost about 6·55 grains in passing at a rate averaging that of eight miles an hour in twelve hours; but a sheet, having the same surface, defended by rather less than $\frac{1}{500}$ lost 5·5 grains; and that like sheets defended by $\frac{1}{70}$ and $\frac{1}{100}$ of malleable iron were similarly worn, and underwent nearly the same loss, that of two grains, in passing through the same space of water. These experiments (the results of which were confirmed by those of others made during the whole of a voyage to and from Heligoland, but in which during the return the protectors were lost) show that motion does not affect the nature of the limits and quantity of the protecting metal; and likewise prove, that independently of the chemical, there is a mechanical wear of the copper in sailing, and which on the most exposed part of the ship, and in the most rapid course, bears a relation to it of nearly 2 to 4·55.

I used the very delicate balance belonging to the Royal Society in these experiments; the sheets of copper weighed between 7 and 8000 grains; and I was fully enabled to ascertain by means of this balance, a diminution of weight upon so large a quantity, equal to $\frac{1}{100}$ of a grain. It was evident from a

very minute inspection of the sheet with the largest quantity of protecting metal, that there was not any adhesion of alkaline or earthy substances to its surface.

Having observed, in examining the results of some of the experiments on the effects of single masses of protecting metal on the sheeting of ships, that there was in some cases in which sheets with old fastening had been used, tarnish or corrosion, which seemed to increase with the distance from the protecting metal, it became necessary to investigate this circumstance, and to ascertain the extent of the diminution of electrical action in instances of imperfect or irregular conducting surfaces.

With single sheets or wires of copper, and in small confined quantities of sea water, there seemed to be no indications of diminution of conducting power, or of the preservative effects of zinc or iron, however divided or diffused the surface of the copper, provided there was a perfect metallic connexion through the mass. Thus, a small piece of copper containing about 32 square inches, was perfectly protected by a quantity of zinc which was less than $\frac{1}{4000}$ part of the whole surface; and a copper wire of several feet in length was prevented from tarnishing by a piece of zinc wire which was less than $\frac{1}{1400}$ part of its length. In these cases the protecting metal corroded with great rapidity, and in a few hours was entirely destroyed; but when applied in the form of wire and covered, except at its transverse surface, with cement, its protecting influence upon the same minute scale was exhibited for many days. A part of these results depend upon the absorption of the oxygen dissolved in the water when its quantity is limited, by the oxidable metal, and of course the proportion of this metal must be much larger when the water is constantly changing; but the experiments seem to show that any diminution of protecting effect at a distance, does not depend upon the nature of the metallic, but of the imperfect or fluid conductor.

This indeed is shown by many other results.

A piece of zinc and a piece of copper in the same vessel of sea water, but not in contact, were connected by different lengths of fine silver wire of different thickness. It was found that whatever lengths of wire of $\frac{1}{300}$ of an inch were used, there was no diminution of the protecting effect of the zinc; and the experiment was carried so far as to employ the whole of a quantity of extremely fine wire, amounting to upwards of forty feet in length, and of a diameter equal only to $\frac{100}{98742}$ of an inch, when the results were precisely the same as if the zinc and copper had been in immediate contact.

Pieces of charcoal, which is the worst amongst the more perfect conductors, were connected by being tied together, and made the medium of communication between zinc and copper, upon the same principles, and with the same views

as those just described, and with precisely the same consequences.

In my first experiments upon the effects of increasing the length or diminishing the mass of the imperfect or fluid conducting surface in interfering with the preserving effects of metals, I used long narrow tubes; but I found them very inconvenient; and I had recourse to the more simple method of employing cotton or tow for this purpose.

Several feet of copper wire in a spiral form were connected with a small piece of zinc wire of about half an inch in length. The zinc and a portion of the copper were introduced into one glass, and the coils of copper wire were introduced into other glasses, so as to form a series of six or seven glasses, which were filled with sea water, and made part of the same voltaic arrangement, by being connected with pieces of tow moistened in sea water.

It was found in these experiments, that when the pieces of tow connecting the glasses were half an inch in thickness, the preserving effect of the zinc in the first glass was no where diminished, but extended apparently equally through the whole series.

When the pieces of tow were about the fifth of an inch in thickness, a diminution of the preserving effects of the zinc was perceived in the fourth glass, in which there was a slight solution of copper; in the fifth class this result was still more distinct, and so on till in the seventh glass there was a considerable corrosion of the copper.

When the tow was only the tenth of an inch in thickness, the preserving effect of the zinc extended only to the third glass; and in each glass more remote, the effect of corrosion was more distinct, till in the seventh glass it was nearly the same as if there had been no protecting metal. All the chemical changes dependent upon negative electricity were successively and elegantly exhibited in this experiment. In the first glass containing the zinc, there was a considerable and hasty deposition of earthy and alkaline matter, and crystals of carbonate of soda adhered to the copper at the surface where it was clean and bright; but in the lower part it was coated with revived metallic zinc. In the second glass the wire was covered over with fine crystals of carbonate of lime; and the same phænomenon of the separation of carbonate of soda occurred, but in a less degree. In the third glass the wire was clean, but without depositions; and the presence of alkaline matter could only be distinguished by chemical tests. In the fourth glass the copper was bright, evidently in consequence of a slight but general corrosion, but with a scarcely sensible deposit; in the fifth, the deposit was very visible; and in the seventh the wire was covered with green rust.

These results, which showed that a very small quantity only of the imperfect or fluid conductor was sufficient to transmit the electrical power, or to complete the chain, induced me to try if copper nailed upon wood, and protected merely by zinc or iron on the under surface, or that next the wood, would not be defended from corrosion. For this purpose I covered a piece of wood with small sheets of copper, a nail of zinc of about the $\frac{1}{200}$ part of the surface of the copper being previously driven into the wood: the apparatus was plunged in a large jar of sea water: it remained perfectly bright for many weeks, and when examined, it was found that the zinc had only suffered partial corrosion; that the wood was moist, and that on the interior of the copper there was a considerable portion of revived zinc, so that the negative electricity, by its operation, provided materials for its future and constant excitement. In several trials of the same kind, iron was used with the same results; and in all these experiments there appeared to be this peculiarity in the appearance of the copper, that unless the protecting metal below was in very large mass, there were no depositions of calcareous or magnesian earths upon the metal; it was clean and bright, but never coated. The copper in these experiments was nailed sometimes upon paper, sometimes upon the mere wood, and sometimes upon linen; and the communication was partially interrupted between the external surface and the internal surface by cement; but even one side or junction of a sheet seemed to allow sufficient communication between the moisture on the under surface and the sea water without, to produce the electrical effect of preservation.

These results upon perfect and imperfect conductors led to another inquiry, important as it relates to the practical application of the principle; namely, as to the extent and nature of the contact or relation between the copper and the preserving metal. I could not produce any protecting action of zinc or iron upon copper through the thinnest stratum of air, or the finest leaf of mica, or of dry paper; but the action of the metals did not seem to be much impaired by the ordinary coating of oxide or rust; nor was it destroyed when the finest bibulous or silver paper, as it is commonly called, was between them, being moistened with sea water. I made an experiment with different folds of this paper. Pieces of copper were covered with one, two, three, four, five, and six folds; and over them were placed pieces of zinc, which were fastened closely to them by thread; each piece of copper so protected was exposed in a vessel of sea water, so that the folds of paper were all moist.

It was found in the case in which a single leaf of paper was between the zinc and the copper, there was no corrosion of the copper; in the case in which there were two leaves, there was a very slight effect; with three, the corrosion was distinct; and it

increased, till with the six folds the protecting power appeared to be lost: and in the case of the single leaf, there was this difference from the result of immediate contact, that there was no deposition of earthy matter. Showing that there was no absolute minute contact of the metals through the moist paper; which was likewise proved by other experiments; for a thin plate of mica, as I have just mentioned, entirely destroyed the protecting effect of zinc: and yet when a hole was made in it, so as to admit a very thin layer of moisture between the zinc and copper, the corrosion of the copper, though not destroyed, was considerably diminished.

The rapid corrosion of iron and zinc, particularly when used to protect metals, only in very small quantities, induced me to try some experiments as to their electro-chemical powers in menstrua out of the contact, or to a certain extent removed from the contact of the air, such as might be used for moistening paper under the copper sheathing of ships: the results of these experiments I shall now detail. A small piece of iron was placed in one glass filled with a saturated solution of brine, which contains little or no air; copper, attached by a wire to the iron, was placed in a vessel containing sea water, which was connected with the brine by moistened tow. The copper did not corrode, and yet the iron was scarcely sensibly acted upon, and that only at the surface of the brine; and a much less effect was produced upon it in many weeks than would have been occasioned by sea water in as many days.

With zinc and brine in the same kind of connexion there was a similar result; but the solution of the zinc was comparatively more rapid than that of the iron, and the copper was rendered more highly negative, as was shown by a slight deposition of earthy matter upon it.

A solution of potassa, or of alkaline substances possessing the electro-positive energy, has nearly the same effect on saline solutions as if they were deprived of air; and when mixed with sea water impedes the action of metals upon them; but if used in quantity in combinations such as these I have just described, in which iron is the protecting metal, it destroys the result, and renders the iron negative. Thus, if iron and copper in contact, or fastened to each other by wires, be in two vessels of sea water connected by moist cotton or asbestos, all the various circumstances of protection of the two metals by each other may be exhibited by means of solution of potassa. By adding a few drops of solution of potassa to the water in the glass containing the iron, the negative powers of the copper in the other glass are diminished; so that the deposition of the calcareous and magnesian earths upon it is considerably lessened: by a little more solution of potassa the deposition is destroyed, but still the copper remains clean. The corrosion of the iron, which

before was rapid, is now almost at an end; and a few drops more of the solution of potassa produces a perfect equilibrium; so that neither of the metals undergoes any change, and the whole system is in a state of perfect repose. By making the fluid in the glass containing the iron still more alkaline, it no longer corrodes; and the green tint of the sea water shows that the copper is now the positively electrified metal; and when the solution in the glass containing the iron is strongly alkaline, the copper in the other glass corrodes with great rapidity, and the iron remains in the electro-negative and indestructible state.

I began this paper by some observations upon the nature of the processes by which copper sheeting is destroyed by sea water, and on the causes by which it is preserved clean, or rendered foul by adhesions of marine vegetables or animals; I shall conclude it by some further remarks on the same subject, and with some practical inferences and some theoretical elucidations, which naturally arise from the results detailed in the foregoing pages.

The very first experiment that I made on harbour-boats at Portsmouth, proved that a single mass of iron protected fully and entirely many sheets of copper, whether in waves, tides, or currents, so as to make them negatively electrical, and in such a degree as to occasion the deposition of earthy matter upon them; but observations on the effects of the single contact of iron upon a number of sheets of copper, where the junctions and nails were covered with rust, and that had been in a ship for some years, showed that the action was weakened in the case of imperfect connexions by distance, and that the sheets near the protector were more defended than those remote from it. Upon this idea I proposed, that when ships, of which the copper sheeting was old and worn, were to be protected, a greater proportion of iron should be used, and that if possible it should be more distributed. The first experiment of this kind was tried on the Sammarang, of 28 guns, in March, 1824, and which had been coppered three years before in India. Cast iron, equal in surface to about $\frac{1}{80}$ of that of the copper was applied in four masses, two near the stern, two on the bows. She made a voyage to Nova Scotia, and returned in January, 1825. A false and entirely unfounded statement respecting this vessel was published in most of the newspapers, that the bottom was covered with weeds and barnacles. I was at Portsmouth soon after she was brought into dock: there was not the smallest weed or shell-fish upon the whole of the bottom from a few feet round the stern protectors to the lead on her bow. Round the stern protectors there was a slight adhesion of rust of iron, and upon this there were some zoophytes of the capillary kind, of an inch and a half or two inches in length, and a number of minute barnacles, both Lepas anatifera and Balanus tintinnabulum.

For a considerable space round the protectors, both on the stern and bow, the copper was bright; but the colour became green towards the central parts of the ship; yet even here the rust or verdigrease was a light powder, and only small in quantity, and did not adhere, or come off in scales, and there had been evidently little copper lost in the voyage. That the protectors had not been the cause of the trifling and perfectly insignificant adhesions by any electrical effect, or by occasioning any deposition of earthy matter upon the copper, was evident from this—that the lead on the bow, the part of the ship most exposed to the friction of the water, contained these adhesions in a much more accumulated state than that in which they existed near the stern; and there were none at all on the clean copper round the protectors in the bow; and the slight coating of oxide of iron seems to have been the cause of their appearance.

I had seen this ship come into dock in the spring of 1824, before she was protected, covered with thick green carbonate and submuriate of copper, and with a number of long weeds, principally fuci, and a quantity of zoophytes, adhering to different parts of the bottom; so that this first experiment was highly satisfactory, though made under very unfavourable circumstances.

The only two instances of vessels which have been recently coppered, and which have made voyages furnished with protectors, that I have had an opportunity of examining, are the Elizabeth yacht, belonging to Earl Darnley, and the Carnebrea Castle, an Indiaman, belonging to Messrs. Wigram. The yacht was protected by about $\frac{1}{125}$ part of malleable iron placed in two masses in the stern. She had been occasionally employed in sailing, and had been sometimes in harbour, during six months. When I saw her in November, she was perfectly clean, and the copper apparently untouched. Lord Darnley informed me that there never had been the slightest adhesion of either weed or shell-fish to her copper, but that a few small barnacles had once appeared on the loose oxide of iron in the neighbourhood of the protectors, which however were immediately and easily washed off. The Carnebrea Castle, a large vessel of upwards of 650 tons, was furnished with four protectors, two on the stern, and two on the bow, equal together to about $\frac{1}{104}$ of the surface of the copper. She had been protected more than twelve months, and had made the voyage to Calcutta and back. She came into the river perfectly bright; and when examined in the dry dock was found entirely free from any adhesion, and offered a beautiful and almost polished surface; and there seemed to be no greater wear of copper than could be accounted for from mechanical causes.

Had these vessels been at rest, I have no doubt there would

have been adhesions, at least in Portsmouth or Sheerness harbours, where the water is constantly muddy, and where the smallest irregularity or roughness of surface, from either wear, or the deposition of calcareous matter, or the formation of oxides or carbonates, enable the solid matter floating in the water to rest. There is a ship, the Howe, one of the largest in the Navy, now lying at Sheerness, which was protected by a quantity of cast iron judged sufficient to save all her copper, nearly fifteen months ago. She has not been examined; but I expect and hope that the bottom will be covered with adhesions, which must be the case if her copper is not corroded; but notwithstanding this, whenever she is wanted for sea, it will only be necessary to put her into dock for a day or two, scrape her copper, and wash it with a small quantity of acidulous water, and she will be in the same state as if newly coppered.

At Liverpool, as I am informed, several ships have been protected, and have returned after voyages to the West Indies, and even to the East Indies. The proportion of protecting metal in all of them has been beyond what I have recommended, $\frac{1}{90}$ to $\frac{1}{70}$; yet two of them have been found perfectly clean, and with the copper untouched after voyages to Demerara; and another nearly in the same state, after two voyages to the same place. Two others have had their bottoms more or less covered with barnacles; but the preservation of the copper has been in all cases judged complete. The iron has been placed along the keel on both sides; and the barnacles, in cases where they have existed, have been generally upon the flat of the bottom; from which it may be concluded, that they adhered either to the oxide of iron, or the calcareous deposits occasioned by the excess of negative electricity.

In the navy the proportion adopted has been only $\frac{1}{250}$ of cast iron, at least for vessels in actual service, and when the object is more cleanness than the preservation of the copper.

It is very difficult to point out the circumstances which have rendered results, such as these mentioned with respect to Liverpool traders, so different under apparently the same circumstances, i. e. why ships should exhibit no adhesions or barnacles after two voyages, whilst on another ship, with the same quantity of protection, they should be found after a single voyage.* This may probably depend upon one ship having remained at rest in harbour longer than another, or having been becalmed for a short time in shallow seas, where ova of shell fish, or young shell fish existed; or upon oxide of iron being formed, and not washed off, in consequence of calm weather, and which consolidating, was not afterwards separated in the voyage. From

* The quality of the copper may be another cause.

what I can learn, however, the chance of a certain degree of foulness, in consequence of the application of the full proportion of protecting metal, will not prevent ship owners from employing this proportion, as the saving of copper is a very great object; and as long as the copper is sound, no danger is to be apprehended from worms.

It ought to be kept in mind that the larger a ship, the more the experiment is influenced by the imperfect conducting power of the sea water, and consequently the proportion of protecting metal may be larger without being in excess.

I have mentioned these circumstances because they apply to ships already coppered, and because I have heard that a Liverpool ship, of which it was doubtful whether the copper was in a state such as would enable her to make another voyage to India with security, has, by the application of protectors of $\frac{1}{70}$, made this voyage,* without apparently any wear of her sheeting; and that she is now preparing with the same protectors to make another voyage.

In cases when ships are to be newly sheathed, the experiments which have been detailed in the preceding pages render it likely, that the most advantageous way of applying protection will be under, and not over the copper: the electrical circuit being made in the sea water passing through the places of junction in the sheets; and in this way every sheet of copper may be provided with nails of iron or zinc, for protecting them to any extent required. By driving the nail into the wood through paper wetted with brine *above* the tarred paper, or felt, or any other substance that may be employed, the incipient action will be diminished; and there is this great advantage, that a considerable part of the metal will, if the protectors are placed in the centre of the sheet, be deposited and redissolved; so there is reason to believe that small masses of metal will act for a great length of time. Zinc, in consequence of its forming little or no insoluble compound in brine or sea water, will be preferable to iron for this purpose; and whether this metal or iron be used, the waste will be much less than if the metal was exposed on the outside: and all difficulties with respect to a proper situation in this last case are avoided.

The copper used for sheathing should be the purest that can be obtained; and in being applied to the ship, its surface should be preserved as smooth and equable as possible: and the nails used for fastening should likewise be of pure copper; and a little difference in their thickness and shape will easily compensate for their want of hardness.

In vessels employed for steam navigation, the protecting

* The Dorothy.

metal can scarcely be in excess;* as the rapid motion of these ships prevents the chance of any adhesions; and the wear of the copper by proper protection is diminished more than two-thirds.

ARTICLE V.

On the Temperature of Mines. By M. P. Moyle, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Helston, March* 1, 1826.

HAVING done me the repeated favour of inserting my communications on the much disputed question as to the heat of the internal strata of the earth, I have to beg a continuance of your kindness in giving place to the following facts in continuation of that subject.

In the first place it may be necessary to state, that during the last summer and autumn, I repeated most of my former experiments on the water in the old and relinquished mines as before stated (vide *Annals*, vol. v. N. S.), and almost precisely with the same results. Suffice it to say on this head, that the greatest heat found in those collections of water from the depth of 20 to 170 fathoms from the surface was 55° Fahr. in Relistian mine, in the parish of Gwinear, while the coldest temperature found was 52° at 134 fathoms in Huel Ann, in Wendron.

I conceived that by selecting a stagnant collection of water in a deep part of a mine at work, the temperature of which spot while it was occupied by the workmen was known, might more effectually give us the true temperature of the surrounding strata, than by any other means. I, therefore, selected a *winze*† at the 110 fathom level, in Huel Trumpet tin mine, in the parish of Wendron. This winze was sunk between four and five fathoms, when it was found necessary to relinquish it from the water being too quick; and until the 120 fathom level was driven far enough under it to drain it of its water.

A hole was bored in the solid granite at the bottom of this winze two feet deep; a thermometer was put into it, and the hole was soon found to fill with water from a natural infiltration without a drop falling into it from above. As this hole filled with water, the thermometer fell to 56°, but in a few hours it

* I have mentioned in the two last communications on this subject some applications of the principle; many others will occur. In submarine constructions—to protect wood, as in piles, from the action of worms, sheathing of copper defended by iron in excess may be used; when the calcareous matter deposited will gradually form a coating of the character and firmness of hard stone.

† A winze is a small shaft sunk simply from one level to another, often required for ventilation, as well as for the judicious working of a mine.

rose to 70°, while the air at the bottom of the winze was 72°. I fastened a line to the thermometer, and allowed it to remain in the hole. The place was now relinquished, and was in the course of a few hours full with water, and great care was taken to prevent any of the water in common to the mine from running into this reservoir. On the following day this water was found at the surface 70°, at two fathoms in depth 68°, and at the bottom 67°; at the expiration of nearly three months, it was thought necessary to examine it again, as the approach of the end of the 120 fathom level might otherwise destroy the opportunity sought. The water was now found at all depths to be 54°. A few weeks after this, the water was found to be sinking, when additional care was taken to prevent any water from falling into the winze; when it had sunk to within two feet of the bottom, the thermometer which was allowed to remain in the hole was suddenly withdrawn, when it was found to be at 54°. Two days after this period, this hole was dry, and showed the temperature of 70°.

Not willing to rely too much on this single experiment, I sought another opportunity of repeating it in Huel Vor tin mine, situated in slate. Here a winze similarly circumstanced to the one just related occurred at the 124 fathom level. This winze was sunk just six fathoms before relinquished, at which time the temperature was 75°; but after being filled with water for about two months, the registering thermometer indicated only 56°; and this possibly might be influenced in some measure by its being found impossible wholly to exclude a fall of water running into it from above.

I do flatter myself that these experiments tend much to strengthen my former assertions of the earth in general possessing and preserving the mean annual temperature of the latitude; and although these experiments give a degree or two above this mark, we cannot but suppose the local causes of heat in a mine at full work must tend to influence the results; but it should be observed that it falls far below what we are taught to expect at these depths, by those holding a different opinion from myself.

Should you, or any of your correspondents, who feel an interest in the present question, be able to suggest any better plans than those already adopted, I shall be most ready to second their views, and will, as far as in my power, carry their intimations into effect. I am, Gentlemen, your obedient servant,

M. P. Moyle.

Article VI.

A simple Method of graduating glass Hydrometers. By Charles Moore, Esq.*

As hydrometers of glass are irregular in shape, they are usually graduated by immersing them in fluids of different specific gravities. But as a considerable number of fluids are required, and as they are liable to change by evaporation, a different method may be found useful.

In trying the specific gravities of fluids by a bottle of known capacity, we compare together the weights of equal volumes; but in using a hydrometer we compare the volumes of equal weights; as the instrument sinks until it displaces a volume of the fluid equal to itself in weight. Hence we derive a method of graduating a hydrometer by the help of one fluid only.

Water, being the standard, is the most convenient, and as its specific gravity is supposed to be unity, we can easily compute how much water is equal in volume to a given weight of another fluid of known specific gravity; or in other words, with what weight a hydrometer should be loaded, in order to make it sink in water at 60°, to the point where that specific gravity should be marked: the weight of such hydrometer when finished being determined.

Let the hydrometer be loaded until it would, if permitted, sink entirely in water, and place in the stem a paper scale divided into small equal parts, taking care that some one known mark corresponds with some remarkable part of the stem. Let it then be suspended from a good small balance, as in taking the specific gravity of solids, and counterpoised by weights in the opposite scale. If a vessel of water be placed under the hydrometer, and weights taken from its counterpoise, it will of course sink and displace an equal weight of water; and in this simple and easy manner we can find the proper places for any required specific gravities; which may be written on a new scale, and put into a similar position.

For example, suppose it were desirable to make a glass hydrometer for acids and saline solutions, beginning with water, and running upwards as high as the length of the stem would allow; suppose also, that the hydrometer when immersed to the upper end of the stem was found to displace x grains of water; it is plain that x grains should be the weight of the instrument when finished: then to find the place where any other specific gravity y should be marked, $y : 1 :: x : \frac{x}{y}$, then $x - \frac{x}{y}$ being added to the counterpoise, the instrument will rise. The vessel should

* From the Dublin Philosophical Journal.

then be lowered a little to bring the beam horizontal, and the mark cut by the water noted for specific gravity y.

In the same manner, by simply adding weights to the scale, as many other specific gravities may be found as are thought necessary; when the scale is to be withdrawn and laid flat, and the intervals measured with a pair of compasses, and transferred to a new scale, the true specific gravities written opposite their proper marks, and the scale put in its place. The instrument may be loaded a few grains heavy, and nicely adjusted in sealing. It will show true specific gravities without referring to a table, temperature being attended to.

If it is desired to adjust a hydrometer for spirits, or fluids lighter than water, then the scale will begin at the lower part of the stem, and the differences of weight being taken from the counterpoise, the instrument will sink.

If it is wished to begin at a specific gravity different from water, suppose at 1·2, for heavy solutions, the only difference will be in loading the instrument. Thus a hydrometer that displaces 300 grains of water must be loaded to weigh 360 grains, that it may stand at the same mark in a fluid whose specific gravity is 1·2. The specific gravity of a body is equal to its weight divided by its volume $\frac{360}{300} = 1{\cdot}2$, then to find any other specific gravity, suppose 1·25, $\frac{x}{y} = \frac{360}{1{\cdot}25} = 288$, and $300 - 288 = 12$. Twelve grains being added to the counterpoise, the instrument will displace only 288, and $\frac{360}{288} = 1{\cdot}25$.

Article VII.

Description of the Process of Amalgamation as carried on in Germany. Extracted from a Letter to John Taylor, Esq. from John Henry Vivian, Esq.

(*Concluded from* p. 201.)

Having thus traced the ore through various mechanical operations, I come to the actual process of amalgamation, or the extraction of the silver by mercury. It will be recollected, that after calcination the ore is raised to the upper floor of the building by means of a shaft and windlass, which is conveniently situated near the division wall between the calcining house and the mills. By its progress through the different sets of screens, mills, and sieves, the ore again reaches the bottom floor, and the same apparatus is employed for raising it to the third story, which is a little below the top of the shaft, where the boxes containing the ore are placed in large iron barrows, and conveyed across

that part of the works containing the screens, to the third division of the main building, or that appropriated to the amalgamation process. It is here deposited in an apartment containing twenty wooden cases, which hold as many charges for the supply of the amalgamation barrels in the room below. The quantity of ore that each case will contain is 10 cwt. which, with the addition of 5 cwt. of mercury, 3 cwt. of water, and a few pounds of iron, constitutes a charge for a barrel.

The barrels are two feet eight inches in length, and about two feet and a half in diameter in the centre. They are well secured by iron hoops, and have a bung-hole, five inches in diameter. A wooden stopper, having a small opening in the centre, fits into the bung-hole, and is fastened down by a band of iron, and a screw. At one end is an iron plate with teeth, by which motion is communicated to the barrels; and each barrel may be detached when required to be stopped.

There are twenty of these barrels in the Freyberg works, arranged in four rows; they are all driven by one overshot wheel; and the whole may be instantaneously stopped, as well as each barrel singly.

I have before stated the weight of the different substances forming a charge for a barrel. I shall now make a few remarks on the manner of filling it. At the bottom of each hopper, or case, in which the sifted calcined ore is deposited, is a wooden tube, to which is attached a piece of sacking, with a mouth-piece of metal that exactly fits the bung-hole of the barrel. The ore is passed from the hopper through this tube into the barrel, by withdrawing a plate at the bottom of the case. To prevent the possibility of any loss of ore, a square frame is placed over the bung-hole, through which the lower part of the tube passes.

The water is supplied from leaden cisterns, one of which, containing exactly the quantity required for a charge, is placed over each barrel. Each cistern has a cock, to which a pipe may be attached, that communicates with the bung-hole of the barrel.

The mercury is conveyed to the barrels from two iron vases placed in an apartment adjoining the amalgamation room, by an iron pipe, which passes between each double row of barrels. Opposite to each barrel is a piece of pipe, so arranged that it may be turned, and by means of a projecting branch in the centre, may serve a barrel on each side; the mercury being conducted into the barrels by a wooden launder placed under the mouth of the pipe, the openings to all the other pipes being at the same time turned upwards. The requisite quantity of mercury for a charge is put into the vase, which has a cock at the bottom; and on a signal being made by the men at the barrels, the cock is turned, and the quicksilver flows along the pipe to the opening communicating with the barrel to be charged.

The iron necessary for the operation is in small bars, which

are constantly kept in the barrels, fresh pieces being added as the metal becomes dissolved.

In charging the barrels, the water is first admitted, then the ore, and with these the barrels are put in motion, and so continued for about an hour and a half. At the expiration of this time, if the ore and water are found to be well mixed, and of a proper consistency, the barrel being somewhat more than two-thirds full, the mercury is added, after which the barrels are made to revolve at the rate of 16 or 18 times in a minute, for about 18 hours. The contents of the barrels are then examined, and if the process be found to have gone on satisfactorily, they are filled with water, and turned slowly for a few minutes. The small aperture in the centre of the bung before mentioned, is then opened, and a wooden pipe fitted into it. This pipe has attached to it at right angles a leather pipe, which is furnished with a cock and a mouth-piece of metal, and may be made to communicate by means of a wooden pipe, with a range of gutters laid under the floor of the room. The apparatus being thus arranged, the bung of the barrel is turned downward, the cock is opened on the silver-holding mercury, which, from its great specific gravity, had collected at the bottom of the mixture, flows through the pipe into the gutters, which lead to a filtering sack, suspended over a stone trough, in an apartment on the ground floor of the fourth division of the building.

The mercury which has not combined with the silver is here separated as it passes through the filter, either by its specific gravity, or by a slight pressure of the hand on the sack whilst the amalgam remains within it. The latter is composed of six parts of mercury and one of silver, and is of such a consistency as to retain an impression made by the finger. When it is found that the greater part of the silver-holding mercury has passed out of the barrel, the cock is turned; the apparatus removed; and the remaining contents of the barrels discharged through the large bung-hole into the gutters, which empty into large tubs in the floor below. Whilst this operation is performing, care is taken not to allow the pieces of iron contained in the barrels to pass through the bung hole. These gutters are then washed with water from a pipe connected with a cistern near the water wheel, and the amalgamation process begins again. The time occupied in filling and emptying the barrels is about six hours, making the whole time of the process twenty-four hours.

I shall now offer some practical remarks on the most important points to be attended to in concluding this operation. And first with respect to the consistency of the mixture in the barrels. This, which may appear at first a trifling object in itself, is of the utmost consequence; for if the mass be in too fluid a state, the mercury will remain at the bottom of the barrel, without being brought into contact with the ore; whilst, on the other hand, if the mass be too thick, the particles of metal will not move freely

through the mass, but will remain in a great degree stagnant. If the mixture is found to be too much diluted, it must be corrected by adding a little ore; if it be too thick, a little water should be introduced. Attention must also be paid to the velocity with which the barrels are made to revolve; as by any material deviation from that degree, which, from experience, has been found best calculated to produce the desired result, nearly the same effect will follow as when the mass is improperly mixed. If the barrels are made to revolve too rapidly, the particles of mercury will collect round the sides. The consistency of the mixture should be occasionally ascertained by stopping a barrel, opening the bung-hole, and dipping a stick into it (if, on withdrawing the rod, the ore drops slowly from it, it is considered in a proper state); and before the process is completed, a sample of the ore should be taken, and tried in a crucible, and the operation continued if found to contain any considerable portion of silver. Attention should also be paid to the state of the mercury in the mixture, which should be minutely disseminated throughout the mass, and the globules not larger in size than the head of a pin.

After the barrels have been a few hours in motion, it will be found that the temperature of the mixture has been considerably increased; and as heat materially assists the amalgamation, this mode of obtaining it by revolving barrels, without the aid of external fires, to which they were formerly obliged to have recourse, is one of the greatest discoveries that were made by De Born. I should have observed, that to De Born, I believe, is also due the merit of organizing, the system of previous calcination of the ore with salt; although the mode of operation was improved by Gellert in the construction of the Freyberg works.

On the opening of a barrel, a strong acid smell may be perceived, and particularly towards the end of the process. This arises from the chemical changes that take place; the muriates which were formed by the calcination are decomposed; the iron unites with the muriatic acid; and the metallic silver, and a portion of copper, or other metals, contained in the ore, combine with the mercury. The muriate of iron, being a soluble salt, becomes dissolved in the water, as do the sulphate and the other salts of soda.

It is obvious that a deficiency of iron in the barrel will prevent the decomposition of the muriate of silver from taking place; care should therefore be had, to keep a proper supply of iron in the barrels.

The next process that requires to be noticed, is the separation of the two metals contained in the amalgam, which is effected by their different degrees of fusibility. The amalgam is exposed in a furnace of a peculiar description, to a certain degree of heat, when the mercury is sublimated, or distilled, and the silver remains.

When the furnace is charged and arranged, the weight of amal-

gam on the plates being about 3 cwt. the fire is lighted over the cast iron cap or bell, and a stream of water is introduced into the cistern which contains the iron pan. The fuel first used is turf, but towards the end of the process they employ charcoal, with which they fill up the whole of the vacant space within the case of the furnace. As the iron bell becomes heated, the mercury is volatilized, and falling to the lower part of the cap becomes condensed, and collects in the iron pan, the water in which is kept cool by the stream before noticed that flows through the wooden cistern in which the pan stands.

In this process every thing depends on a gradual increase of temperature, and on a proper degree of heat being ultimately given. If the furnace is heated too rapidly, a portion of the mercury will remain united with the silver, which must infallibly happen if any part of the compound is fused; or too great a fire may also occasion another bad effect—a portion of silver may be sublimated with the mercury. On the other hand, a sufficient degree of heat must be given to separate all the mercury; for if any portion remain combined with the silver, it would be lost in the testing process that follows. The distilling process lasts from eight to ten hours; the person who attends upon it knows when the whole of the quicksilver is extracted, by the cessation of the sound made by the drops of that metal falling into the water as it is condensed. On the following day, when the furnace has become thoroughly cool, the outer door is opened, the cap raised, the mercury in the receiver collected, and the silver on the plates removed. The mercury is raised by a windlass, in small boxes fitting into a square case or kibble, containing all together about 5 cwt. and deposited in the iron vases in the apartment over the distilling house, and adjoining the amalgamation room. It may contain a small portion of silver, but that is of no importance, as it is again used for amalgamation with that part of the quicksilver taken from the barrels, which had passed through the filters as before described. The loss of mercury in the distilling operation is about one-third of a *loth* to the mark of fine silver obtained, which is nearly two per cent. on the quantity used. The silver on the plates should be in ragged porous pieces. Some specimens are in fancy forms, resembling trees, &c.; and may be purchased at the works on paying the value of the silver. The metal is for the most part of the colour of silver; but it is frequently tarnished with a yellow or brown tint. It contains in fine silver from 12 to 13 *loths* in the mark, or about 75 per cent. The other metals that remain combined with it are such as were contained in the ore; as copper, cobalt, nickel, antimony, &c. To reduce the amalgamated silver to a compact state, and of uniform fineness, the pieces taken from the plates are melted in a Passau crucible previous to being refined, which is done at the neighbouring smelting-work. The deliveries from the amalgamation-work to

the smelting-work take place at the end of each fortnight. The contents of each delivery in fine silver is generally from 1100 to 1200 marks.

I shall now proceed to describe the washing process, or that practised on the residuum in the barrels after the silver-holding mercury is removed. This body of stuff, as I before observed, is passed down into large vats on the ground floor of the building. It consists of the earthy parts of the ore, in which are some small particles of mercury intermixed, and water holding sulphate of soda, and other salts in solution. The main object here is to collect the metallic particles from the mass of ore; and to effect this, fresh portions of water are added to the stuff discharged from the barrels, and then worked about in large vats or tubs. In each vat works an upright spindle with arms, and attached to the latter by several plates or bars of iron. These vats are furnished with eight plugs one above the other; so that as the heavier metallic particles subside, the waste or earthy matter may be drawn off at the different plugs in succession, beginning with the top. Those near the bottom are not opened until the mercury there collected is removed, which only takes place once in three or four weeks. The vats are about six feet in diameter at the top, and from five to six feet in depth. One of these vats is attached to each range of five barrels. The time allotted for the washing process is twelve hours.

The principal points to which the attention of those conducting this operation is to be directed are, first, the careful examination of the stuff, previous to its being let off at each plug, for particles of mercury: this may be done by diluting a small quantity in a wooden trough. Secondly, so to regulate the quantity of water in the mixture, that the coarser particles of the ore may not subside with the mercury, which will be the case if the stuff be too much diluted; and at the same time to avoid having the mercury kept in suspension in the mass, which must happen if it be not diluted in a sufficient degree; for it is evident that unless the particles of mercury be disentangled from the earthy matters, they will not subside by their greater specific gravity, which is the object of the operation. Thirdly, to regulate properly the velocity of the machinery; for too quick a motion will prevent the particles of mercury from subsiding; whilst, if the motion be too slow, the heavier particles of the ore will fall to the bottom with those of the mercury. The reason for having the rods which connect the cross bars of the revolving machine in the vat of iron is, that any mercury which may have been oxidized during the amalgamation process may be reduced to the metallic state, the oxygen possessing a great affinity for iron. A portion of mercury occasionally appears in this last-described process, and the other processes in which it is brought in contact with water, in the form of a white foam floating on the surface. This may arise either from a portion of

the metal being oxidized, or from its particles, finely disseminated, becoming intimately mixed with the particles of water. In this last case the foam should be left to settle, when the quicksilver will collect together. The waste from the vats is conducted through underground gutters out of the works, and received in pits. In these the earthy matter is allowed to subside, and the saline solution is drawn off to be employed elsewhere. As a check on the operation in the works, the deposit which is received into the waste pits should be occasionally assayed for silver, as well as examined for particles of mercury. The contents of the waste pits are thrown from thence into the river, and carried off by the first flood. I believe it is the custom at Freyberg to assay the residue from each vat, and I understood it usually contains a small portion of silver; perhaps from $\frac{3}{16}$ to $\frac{1}{4}$ of a *loth* in the *centner*.

I recollect its being mentioned to me in 1815 as a singular circumstance, that the ten barrels on the north side of the amalgamation-room invariably yielded a cleaner residue than those on the south side, although the charges and treatment were in every respect the same. As I felt incredulous as to the correctness of this assertion, I examined the assay books at the works; and it certainly did appear as stated, that the residuum from the barrels on the south side contained invariably a small quantity of silver more than those on the north side.

It now only remains for me to make a few remarks on the uses to which the saline solution drawn off from the waste pits may be applied. Various experiments have been made with a view to ascertain the most advantageous mode of using this liquid. It has been sometimes applied to the preparation of manure, by decomposing the sulphate of soda with lime, and thus forming an insoluble salt, or artificial gypsum: and it was remarked to me by Prof. Lampadius, that the action of this salt as a manure appeared to be increased by the iron precipitated from the muriate, by its acid uniting with the soda of the Glauber salt, which becomes highly oxidized by exposure to the atmosphere. During my last visit to Freyberg, Prof. Lampadius was engaged in some experiments for applying the sulphate of soda, obtained by evaporation, to glass-making, instead of potash; and from some specimens of white glass, &c. showed me, it appeared that the experiment might possibly be successful.

The general arrangement of the interior of the principal building of the Freyberg amalgamation-work may be collected from the description given of the different processes; from which it will appear that much labour and expense may be saved by facilitating the communications between the different parts of the works. By means of a shaft and machine for raising the ore to the different stories of the building, and apertures in the floors for letting it down, as it may be required in the different

processes, waste is avoided, and the temptation to pilfering of the valuable metals by the workmen is diminished. A short review of these arrangements may not, therefore, be improper, as thereby the connexion between the different parts of the establishment may be better understood.

The works consist of three main buildings, which form three sides of a quadrangle; the fourth side of which forms the entrance. In the centre is the forcing engine, for the purpose of supplying water in case of fire. One of the three buildings is much shorter than the other two, and contains only furnaces and a charging floor. The others contain each the same number of furnaces and conveniences for mixing and charging the ore, and in addition thereto machinery and apparatus required in the other processes. The principal building containing the barrels may be in length about 200 feet by about 48 feet in breadth. It is divided by stone walls into four divisions. The first division, which is by far the largest, contains on the ground-floor a set of calcining furnaces; over these is a charging floor, and above that a salt magazine. Next to this is the part used for milling. This contains on the upper, or fourth story, a coarse riddle, and below it a fine riddle. Under these are five mills, extending with the sieves attached thereto to the floor. The third division contains, on the third story, a floor from whence the amalgamation barrels are charged; on the second floor the barrels; and under these, on the ground floor, the washing tubs. In the fourth division is a storehouse for quicksilver; on the second story, and under this, the four distilling furnaces. The third and fourth stories of the building, it is to be observed, are in the roof, which is made very steep, the walls of the work being only two floors in height.

In describing the machinery, it is not necessary to be very minute. It consists of cog wheels and wooden lanterns, an arrangement very commonly adopted on the continent, but evidently inferior to the machinery of this country, as the bearing of the wooden cogs on the round staves of the hollow lanterns cannot be as true as when two well constructed tooth wheels work together; nor are the face wheel and the lantern pinion at all equal to our bevil gear. The construction of the mills is far less simple than that of our grist mills, in which several pairs of stones are often driven by a single spur wheel. The whole of the machinery is driven by water. In the principal building before referred to are two large overshot wheels, of which one works the screens, mills, and sieves, and the other the barrels.

The arrangement for the latter is as follows: Attached to the shaft of the barrel water wheel is a large cog wheel that works two lantern pinions placed above it. On the shaft, from each of these pinion wheels are five small cog wheels, so arranged that

each wheel works two barrels. Thus the whole of the twenty barrels are made to revolve by one water wheel, which is about 28 feet in diameter. The washing apparatus under the barrels is worked by a separate water wheel. Every part of the machinery is carefully attended to, and kept in the very best order.

I shall conclude by a general recapitulation of the power of the work, and the extent of the operations:—

The three buildings just described, with one or two of a smaller description that are contiguous to them, contain the following apparatus.

A large ore house.

12 calcining furnaces arranged in three sets, with a charging floor and salt magazine to each.

2 coarse riddles.

2 fine riddles.

14 mills.

20 amalgamation barrels.

4 mercury distilling furnaces.

4 washing vats.

The quantity of ore operated on annually amounts to about 3000 tons; and its contents in fine silver may be estimated at from 28,000 to 29,000 marks.

The annual consumption of mercury is calculated at 25 cwt. which will be in proportion to the silver obtained as about 1 to 5, as mentioned by Humboldt. The mercury used is brought from Prague, and is probably from the mines at Idria.

The salt employed, in the proportion of 10 per cent. on the quantity of ore amalgamated, may be taken at 300 tons. It is obtained from the Saxon salt-works at Arten.

The consumption of iron in the barrels is from three to four tons per annum.

The quantity of the various substances used as fuel, Lampadius calculated, in 1802, to be equal to $5\frac{3}{4}$ cubic feet of charcoal to each quintal of ore; and the amount of wages on 100 quintals of ore at $17\frac{1}{6}$ rix-dollars. I do not recollect having seen any calculation of the total cost per ton of ore, incurred in extracting the silver by mercury; but, I believe, the charges are nearly the same as in smelting. The chief advantage of amalgamation at Freyberg is the saving of fuel; for if all the ores of the district were smelted, the quantity of charcoal required would be so great that the price of that article would necessarily advance.

I have thus I believe noticed the most material points relating to the Freyberg amalgamation-work, and the operations carried on therein. I have only again to repeat, that I shall feel truly gratified if these remarks should prove of use to your friends engaged in mining speculation on the other side of the Atlantic. Should you conceive that any further information I can give may be of service, I beg you will freely command me.

Article VIII.

Account of a Mineralogical Excursion to the Counties of Galway and Mayo. By Sir Charles Lewis Gièsecke.* (Communicated by a Member of the R. D. S.)

County of Galway.

I left Dublin on the 14th of August, in conformity to the directions of the Royal Dublin Society, and proceeded to the County of Galway. I made my first excursion to Marble Hill, and went the following day towards Woodford, on the new road towards the Shannon, constructed by Mr. Killaly. Part of this country consists of diluvial land, in which I found immense blocks of beautiful conglomerate, consisting of fragments of rose-red quartz and hornstone, of a dark leek-green jasper, and large blocks of mica slate. Near Woodford there is a very extensive stratum of meadow iron ore, which was worked by English miners about a century ago, and afforded very good cast-iron, which was shipped on the river Shannon. The turf, which covers the stratum of the iron ore occurs in enormous abundance; it approaches to moor-coal, and is of the best quality.

The day after my arrival in Galway I went on an excursion to Cunnamara. The black limestone is visible every where along the road to Oughterard, a small town five miles from Galway, which is visited very much during the summer season by the gentry from Galway and its neighbourhood, on account of its salubrious spa. As soon as you leave Oughterard, the junction of the granite with the limestone is visible. In the evening I arrived at Ballinahinch, the residence of Thomas Martin, Esq. who received me with his usual kindness.

He accompanied me on the following day to what is called the green marble quarry, but which is rather a quarry of precious serpentine, belonging to his estates. It is situated in a valley extending from the northward to the west peak of Lettery, to as far as the middle of the place called the Twelve Pins, a series of very acuminated coherent mountains. I found following up the river, or rather torrent, traces of this serpentine at a distance of a mile from the quarry. The rivulet, which took a serpentine direction, has disclosed to the eye extensive strata of most beautiful granular marble, of a pearl white colour, mixed with rose-red, yellowish-red, blood-red, and bluish-grey. It alternates with greenstone. The large serpentine quarry, where Mr. Martin keeps from 150 to 170 labourers employed in blasting, cutting, and sawing the immense blocks, is of an extraordinary extent, and seems to be inexhaustible. The serpentine, similar

* From the Appendix to the Report of the Proceedings of the Royal Dublin Society, March 2, 1826.

to the Serpentino Antico of Italy, is mixed with steatite, fine granular limestone and stripes of asbestus, and occurs in blocks sometimes of the length of 12 and 13 feet, and three or four tons in weight. It is impossible to describe the immense varieties of delineations and shades and colours of this beautiful stone, which attract the eye of the beholder; the serpent-like delineations of some of them must excite particular admiration. Others are coloured in spiral forms, others are dotted and spotted with different shades of green, grey, and yellow. Solid masses of an enormous size may be raised. Mr. Martin has already quarried out an immense quantity, part of which is cut in slabs for tables, and which are ready for sale. He has also made a road from the quarry to the port, a distance of six miles, but it would require a railroad for large blocks. Higher up towards the northwest of the Twelve Pins, there occurs a kind of serpentine which contains but very little of the grey and white granular limestone. The limestone seems here rather to form small beds in the serpentine. I found along the river large blocks of granite with imbedded and nine-sided prisms of tourmaline of a pitch-black colour, similar to that of Killiney. The granite of the country is fine and coarse granular, very durable; and abounds in felspar of a flesh-red and reddish-brown colour; beds of felspar occur in it. Another quarry of serpentine, which was worked formerly by Mr. D'Arcy, of Clifden, is now in the possession of the Hibernian Mining Company. On another day I went by a boat to the black marble quarry four miles from Galway, which is worked by Mr. Ireland, and to another at Merlin Park, the property of Mr. Blacke: the latter is of the most beautiful jet black colour, and very transparent; the former is rather of a slaty structure, and on that account easier worked. It contains numerous petrefactions, particularly of gryphites, and is sometimes intersected by small veins of fluor. As the quarry is situated close to the shore, the marble is carried in great quantities to the neighbouring country, and also to London.

County of Mayo.

I entered the County Mayo on the road leading through the little town of Cong, a place remarkable on account of its subterraneous springs and extensive caves, through which rapid streams run in different directions. The rocks consist of fine granular slaty sandstone, in which there are found in nests most beautiful specimens of perfectly transparent glass-white and yellowish-white calcareous spar of a rhomboidal shape, not inferior to those known by the name of Icelandic double refracting spar. The walls and the roofs of the caves are covered and decorated with calcareous stalactites of greyish-white and yellowish-grey colours, and of different shapes. In the river close to a mill there is a whirlpool, and near to the church fine

ruins of a monastery. I proceeded by the Neal and Ballinrobe to Westport, where I arrived the same evening. The rocks all along the road consist of greyish-black compact limestone, interspersed with spots and veins of white calcareous spar.

The following morning I went to a lead mine called Sheffry, twelve miles to the south-west of Westport, which is worked at present by the Hibernian Mining Company, and is on the estate of the Marquis of Sligo. The mountains consist of smoke-grey and greenish-grey clay slate, alternating with chloriteslate very much decomposed on its surface, and intersected by veins of yellowish-white common quartz of different breadths. The vein of lead ore, which runs in a south-westerly direction, is opened at a distance of about 400 paces from the new road constructed by Mr. Bald, where has been driven a level of about twenty-seven fathoms. The lead ore (common coarse granular galena) runs through the clay slate, which is of an ash-grey colour, and somewhat decomposed. It occurs partly massive, partly crystallized in cubes, and is accompanied by copper pyrites, partly massive, partly crystallized in rhomboids and three-sided pyramids, sometimes pavonized. The other minerals, which are found in the vein along with the ore, are common white quartz, massive, and crystallized in six-sided prisms, brownspar of a yellowish-white colour, prase, yellow iron ochre, barytes, and a yellowish-green earthy substance. There are scattered about in the diluvial land fragments of leek green flint, hornstone, and common quartz. I went along the road through the chain of mountains, which consists of clay slate, to the fishing lodge of the Marquis of Sligo down to Killery bay, which separates the Counties of Galway and Mayo.

The rock that borders the coast of the county of Mayo in Killery Bay is hornstone porphyry, of a liver-brown colour, with small white and reddish-white crystals of felspar, and veins of green talc. It is covered in some places by a coarse quartz-conglomerate. I was informed that not far from the village Bonduragh there was an iron mine worked about thirty years ago.

On one of the following days I made an excursion to the Reek and Croagh Patrick, an extensive mass of rocks, which consist of mica slate, with overlying common serpentine. In the latter there is imbedded common leek-green talc, and greyish-green amianthus.

Achill Island.

I proceeded in a boat belonging to the Marquis of Sligo, who very kindly assisted me in my excursion, across Clew Bay to Achill Island, twenty miles from Westport. The vast mountain tracts of this island consist of mica slate of a very fine slaty texture, with beds of the rock, called by the late Werner white-

stone. I stopped for a night in a cabin, and made, on the following day, accompanied by some inhabitants of the island, a pedestrian excursion over the mountains, to the most western point of it, called Achill Head, a distance of ten miles, where I arrived late in the evening, and remained in the cabin of a water guard. The following morning I went to the place described to me, where the mineral, known here by the name of Achillstone, is to be found. The substance so called is an amethyst-quartz of different shades of colour, passing from amethyst-blue into violet-blue and rose-red, and forms a vein of from one to two feet broad, which is on its outgoing very much scattered and broken into pieces. It runs in a southerly direction through mica-slate of a coarse slaty undulated texture, which contains much quartz. The amethyst bordering the matrix is in bacillar and cuneiform district concretions, which terminate in six-sided pyramids. In some places are found broken particles of rock crystal and common quartz of a greyish-brown tinge. At the foot of the mountain a very coarse granular iron-shot quartz conglomerate, mixed with brown common jasper and hornstone, covers part of the mica-slate, which is converted into a grey greasy earthy substance, not unlike to powdered graphite. I could not trace any other metallic substance on the spot except copper-pyrites, disseminated in common quartz.

Having now been nine weeks on my tour, and having latterly been exposed to a good deal of fatigue, and the weather having broke, I was reluctantly compelled to relinquish my examination of the remainder of this interesting country.

To this I annex a descriptive catalogue of the specimens, 105 in number, which I obtained on my journey, and which I propose should form a part of the new Irish collection.

A Descriptive Catalogue of the Mineral Substances found in the Counties of Galway and Mayo.

County of Galway.

No.

1—2. Quartz conglomerate, of rose-red and blood-red colour, with hornstone, from Woodford.

3—4. Dark leek-green jasper, from the same place.

4—6. Mica-slate, approaching to avanturine, from ditto.

7—10. Bog iron-ore (meadow iron-ore, Jameson), of a blackish and yellowish-brown colour, from a place near Woodford.

11—12. Black turf, approaching to moor coal, covering the bog iron-ore.

13—14. Pearl-grey limestone, from Marble Hill.

15—18. Dark velvet-black limestone, with fragments of shells,

transmuted into calcareous spar, from Oughterard, Cunnamara.

19—20. Granite from the junction with the limestone, near Oughterard.

21—22. Mica-slate, from the mountains between Oughterard and Ballinahinch.

23—24. Granite, from the same place.

25—28. Fine granular limestone (marble), of pearl-grey colour, mixed with rose-red, from the rivulet near the Twelve Pins.

29—30. Precious serpentine, of yellowish-green colour, cloudy, mixed with pearl-grey calcareous spar, from Ballinahinch.

31—32. Ditto, of dark leek-green colour, undulatingly striped, with some white calcareous spar, from the same place.

33—34. Ditto, of leek-green colour, mixed with brown and grey, from ditto.

35—36. Ditto, of sulphur-yellow colour, mixed with grey and white, from ditto.

37—40. Four other beautiful varieties, with cloudy delineations, from ditto.

41—42. Tourmaline, of pitch-black colour, imbedded in granite, from Penacuola.

43—44. Chalcedony, passing into flint of yellowish-grey colour, from the same place.

45—46. Black limestone, from Ireland quarry.

47—48. Violet-blue fluor, in black limestone, from ditto.

49—50. Black limestone, with gryphites, from ditto.

County of Mayo.

51. Glass white double refracting spar, transparent, from Cong.

52. Ditto, yellowish-white, from the same place.

53—54. Yellowish-white and greyish-white stalactite, from the cave, near Cong.

55—56. Greyish-black limestone, with traversing veins of calcareous spar, from Ballinrobe.

57—58. Smoke-grey clay-slate, from the lead mines, near Sheffry.

59. Chlorite-slate, of leek-green colour, with much quartz, from the same place.

60—64. Massive galena, of lead-grey colour, of different texture, from the same place.

65. Common galena, crystallized in cubes, with clay-slate, from the same place.

66—68. Copper pyrites of brass-yellow colour, accompanying massive galena, with clay-slate, from the same place.

69—70. Ditto, in rhomboids, and three-sided pyramids, accompanied by galena, massive quartz.
71—72. Common quartz, pearl-grey, from the same place.
73. Ditto, somewhat iron-shot.
74—75. Calcareous spar, of yellowish-white colour, massive, with copper pyrites.
76. Calcareous spar, in six-sided prisms, accompanied by copper pyrites.
77. Brown spar, massive, and in small rhomboids of yellowish-white colour.
78. Brown spar, of rose-red colour, with some calcareous spar and pyrites.
79. Leek-green quartz (prase), mixed with common quartz.
80. Yellow iron ochre, earthy.
81. Massive barytes, of pearl-grey colour.
82. Earthy chlorites, of yellowish-green colour, with quartz.
83. Leek-grey flint.
84. Hornstone porphyry, of liver-brown colour, with felspar.
85. Coarse quartz conglomerate.
86—88. Common serpentine, of leek-green colour, with common talc, from the Reek.
89. Slaty talc, of leek-green colour, from ditto.
90. Amianthus, forming narrow veins in common serpentine, from ditto.
91. Asbestus, of pearl-grey colour, from ditto.

Achill Island.

92. Mica-slate, with white quartz, very thin, slaty, and silver-white mica.
93. White stone, of pearl-grey colour.
94—95. Amethyst quartz, of violet-blue colour in bacillar distinct concretions, with mica.
96. Amethyst quartz, of amethyst-blue colour, cuneiform fragments, with mica.
97—98. Amethyst quartz, of pale violet-blue, and amethyst-blue colour, in isolated crystals, with some mica.
99—104. Varieties of colours of amethyst quartz.
105. Quartz conglomerate, of coarse granular construction.

Article IX.

On the Poison of the Toad.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

Having called to make inquiries about a sick friend, I found the second volume of Paris's and Fonblanque's Medical Jurisprudence lying upon his table; and wishing to kill a little time, I took it up, and accidentally opened it at p. 139, where, to my astonishment, I found an account of the supposed poison of the toad, very similar to that which I had just read in the *Annals*, as the results of a paper lately given by Dr. Davy to the Royal Society. If it is not an impertinent question, may I be allowed to ask, whether it is usual to present any memoir to that Society which contains matter already in print? I have sent you the extract to which I allude; and I think by giving it a place in your journal, your readers will be gratified; for it affords a very good account of the question at issue. In making this request, I hope I do not intrude. I remain, your constant reader, P. P.

"With respect to the poisons of *Locusta*, all cotemporary writers speak of the venom of the toad as the fatal ingredient of her potions, and in the Alexipharmaca of Dioscorides we find the symptoms described, which are said to be produced by it;* but what is very extraordinary, the belief of the ancients on this matter was all but universal. Pliny is express on the subject; Ætius describes two kinds of this reptile,† the latter of which, as Dr. Badham has suggested, was probably the frog, as well from the epithet, as that he ascribes deleterious powers only to the former. It is scarcely necessary to observe that this ancient belief has descended into later times; we find Sir Thomas Browne treating such an opinion as one of the vulgar errors; and we have before alluded to the legend of king John having been poisoned by a wassail bowl in which matter extracted from a living toad was said to have been infused. In still later times, we have heard of a barrel of beer poisoned by the same reptile having found its way into it. Borelli and Valisnieri maintain that it is perfectly harmless, and state that they had seen it eaten with impunity. Spielman‡ expresses the same opinion, "*Minus recte itaque effectus venenati a bufonibus metuuntur.*" Franck,§

* "επιφερεν οιδηματα σωματος, μετα ωχροτητος επιτεταμενης. δυσπνοειν και δυσωδια οζωδεναι το στομα, και λυγμος αυτοις επεται, ενιοτε δε και σπερματος απροαιρετος εκκρισις."

† 1. κωφος η αφθογγος; 2. φωνητικος.

‡ Instit. Mater. Medic. p. 176.

§ Manuale di Tossicologia, p. 79, 245.

on the contrary, accuses Gmelin of too much precipitancy in rejecting the belief respecting toad-poison.* Modern naturalists recognise no poisonous species of toad; even the most formidable of the species, to appearance, that of Surinam, is said to be perfectly harmless.

"If we may venture to offer a conjecture upon this subject, we are inclined to consider the origin of this opinion to have been derived from the frequency with which the toad entered into the composition of spells or charms, into philtres or love potions, and which, like the bat and the owl, most probably derived its magical character from the gloom and solitude of its habitation. Shakspeare has accordingly introduced this reptile into the witches' enchanted cauldron, in Macbeth.

> "'Round about the cauldron go;
> In the poison'd entrails throw.
> Toad that under coldest stone
> Days and nights hast thirty-one
> Swelter'd venom sleeping got,
> Boil thou first i' the charmed pot!'"

"This opinion receives further strength when it is considered how frequently poisons were administered under the insidious form of charms or incantations.†

"It has, however, been shown by late experiments that the toad has, under particular circumstances, the power of ejecting from the surface of the body an acrid secretion which excoriates the hands of those that come in contact with it; and this fact may perhaps have assisted in supporting the general belief respecting the poisonous nature of this reptile. Pelletier has ascertained, that this corrosive matter contained in the vesicles which cover the skin of the common toad (*Rana Bufo*), has a yellow colour, and an oily consistence, and to consist of,—1st, an acid partly united to a base, and constituting $\frac{1}{20}$th part of the whole. 2d, very bitter fatty matter. 3d, an animal matter bearing some analogy to gelatine.—(Medical Jurisprudence, vol. ii. p. 139.)

[*Note.*—We have received the above letter from a correspondent, which, with the extract from Dr. Paris's Medical Jurisprudence alluded to, we have no hesitation in laying before our readers. At the same time we must observe, in justice to Dr. Davy (and we are certain our correspondent will agree with us), that no suspicion of intentional plagiarism can attach to him, as

* See also Istituzioni di Med. For. di G. Tortosa, vol. ii. p. 67, and authorities there cited.

† This fact may be illustrated by ancient as well as modern records; from the poisoned tunic of the Centaur Nessus, to the treacherous powders of the diabolical Mary Bateman.

author of the paper, On the Poison of the Toad, lately read before the Royal Society (see *Annals of Philosophy*, for Feb. p. 137.) Dr. Davy has been absent from England, on professional duty, for some years, and is still abroad, we believe at Malta; and it is not extraordinary that, so circumstanced, he should not have had access either to Dr. Paris's work, or to any other in which Pelletier's experiments on the poison of the toad are recorded.—C. and P.]

Article X.

On the Reciprocal Decomposition of Bodies. By M. Gay-Lussac.*

We are indebted to Berthollet for the important law that bodies having analogous properties mutually displace each other from their combinations, and that the principal causes which limit their separation are volatility and insolubility. Berthollet has not, perhaps, sufficiently developed the consequences of this law; but it is easy to predict them in every particular case.

When two acids act on a base, and the whole remains in solution, the base is divided between them, not according to their ponderable quantity, but to the number of their atoms; and it appears that its affinity for each acid has generally an important share in the phænomenon. In order to the division of the base, it is enough that the acids, whatever may be their difference in respect to volatility or solubility, remain in solution; for in that case they must behave as if they possessed those two properties in an equal degree.

Suppose, for instance, that we pour an excess of nitric acid on chloride of sodium, the mixture will immediately contain hydrochloric acid and chlorine, and if it be heated, the chloride will soon be converted into nitrate of soda. In making the converse of this experiment; that is, in treating nitrate of soda with an excess of hydrochloric acid, we shall convert it into chloride of sodium. These reciprocal decompositions are very easy, and by converting two nitrates into chlorides, we can determine the proportion in which they were mixed; all that is necessary is to know the weights of the two nitrates and the two chlorides, and the atomic weight of each salt. All the chlorides are not decomposed by nitric acid with the same facility; chloride of silver, which is completely insoluble in water and acids, is not at all attacked by it; and chloride of lime is not so readily attacked as the chloride of potassium or sodium. But we must also observe, that we are now comparing compounds (chlorides and nitrates) which are not at all analogous, and that we can only

* From the Annales de Chimie.

apply the law of which we have spoken by supposing that the chlorides remain indifferently in the solutions in the state of chlorides, or that of hydrochlorates; which is not always the case. Sulphuric acid at common temperatures partially separates the boracic and arsenic acids from their combinations; but at a high temperature, it is, on the contrary, expelled by them.

Nitric and hydrochloric acids decompose the fluorides, and in its turn hydrofluoric acid decomposes the nitrates and chlorides.

Acetic acid decomposes several chlorides, and reciprocally hydrochloric acid decomposes the acetates. Many other vegetable acids, and particularly lactic acid, present analogous phænomena.

Gases soluble in water, and which are separable from it by a vacuum, are all expelled from that liquid by another gas passed into it in excess.

One might quote a multitude of similar facts, but we shall confine ourselves to the mention of the decomposition of hydrosulphates by carbonic acid, and that of carbonates by hydrosulphuric acid (sulphuretted hydrogen), respecting which M. Henry, the younger, has lately been engaged in a very long investigation, to demonstrate what a little reflection would have easily convinced him of, on the laws established by Berthollet.

Bicarbonate of potash, for instance, exposed in solution to the contact of the air, loses a portion of its acid, and acquires the property of precipitating sulphate of magnesia. If we pass a current of hydrosulphuric gas into it, whose acid properties, as is well known, are nearly the same as those of carbonic acid, a portion of carbonic acid will necessarily be liberated; and as it will be gradually expelled by the current of hydrosulphuric gas, the bicarbonate remaining in solution will always be under the same circumstances as to its decomposition, which, therefore, by degrees, will become complete.

In like manner, if a current of carbonic acid be passed into a solution of a hydrosulphate, the latter will be partially decomposed, and the hydrosulphuric acid set free, being carried off by the current of carbonic acid, the decomposition of the hydrosulphate will continually go on till it is complete.

We must observe that these decompositions require a much larger quantity of acid than would be necessary to saturate the base; for the eliminated acid cannot escape from the solution, but by the action of a great excess of the acid which takes its place, according to the theory of vapours.

Moreover if the carbonate and hydrosulphate be not in the state of bisalts, neither the one nor the other will begin to let go their acid till they have attained that state. M. Henry has observed that the insoluble carbonates experience only a very

slight decomposition by the action of hydrosulphuric acid, as may be easily imagined; but, what is not so obvious, is, that the carbonates, according to the same observer, are more difficultly decomposed by hydrosulphuric acid than the hydrosulphates by carbonic acid.

Article XI.

Analyses of Books.

1. *Philosophical Transactions of the Royal Society of London, for 1825. Part II.*

(*Concluded from* p. 223.)

In the next number of the *Annals*, we shall give a general account of the present state of science, on the subject of the magnetism developed in various substances by rotation, embracing the principal contents of the four papers on that subject contained in this part of the Philosophical Transactions. Articles XIV. and XVI. therefore, being the papers on the magnetism of iron arising from its rotation, by Messrs. Barlow and Christie, we shall at present pass over; and Art. XV. is the paper by Sir H. Davy, which we have inserted in the present number.

XVII. *Some Account of the Transit Instrument made by Mr. Dollond, and lately put up at the Cambridge Observatory.* By Robert Woodhouse, Esq. AM. FRS.

The dimensions of this instrument are nearly the same as those of the Greenwich transit made by Mr. Troughton.

	Ft.	In.
Its focal length is.	9	10
Its aperture	0	5
The length of the axis between the piers	3	6

The weight of the instrument is 200 pounds. It was counterpoised; but after repeated trials, Mr. Woodhouse has been obliged to abandon the counterpoises for the present, for instead of relieving the instrument, they rendered it unsteady. The whole lengths (two inches) of the pivots rest on the Ys. Seven fixed wires are placed in the focus of the object glass, and two other wires moveable by a micrometer screw; the interval of which wires is equal to the interval between any two of the fixed wires, and, *equatorially*, is $17^{s}{\cdot}88$. Two small graduated circles, with their spirit levels, are fixed near to the eye piece, for the purpose of finding a star's place in the meridian. Each circle is furnished with two verniers; one for polar, the other for zenith distances.

A remarkable effect on the instrument was observed in levelling, which is thus described by Mr. Woodhouse:

"The tube of the telescope is braced to the axis by four tubes. The stations of the two persons who level are opposite, and contiguous to the south-west and north-east braces. Being in the constant habit of examining the meridian mark, in order to know what degree of stability the instrument possesses, I found, after levelling, that the south meridian mark was to the east of the middle wire. In about 10 minutes the middle wire returned to the meridian mark, and bisected it. I noted this circumstance, a second, third, and fourth time, and then began to inquire whether I had conjectured rightly in attributing it to the expansion of the tubes or braces. For this end, I placed a heated blanket across the south-west and north-east braces, and found the meridian mark deviating to the east of the middle wire: a contrary effect was produced by placing the blanket across the south-east and north-west braces. In these trials the object glass was towards the south: contrary effects took place when it was turned to the north.

"As yet I am unable to say whether or not the sun's rays falling on the braces, during an observation of his transit, affect the accuracy of the observation. I am inquiring into that point, and have ordered a screen to be made to protect the braces from the rays of the sun."

XVIII. *On the Fossil Elk of Ireland.* By Thomas Weaver, Esq. MRIA. &c.—(See *Annals* for June, 1825.)

XIX. *Microscopical Observations on the Materials of the Brain, and of the Ova of Animals, to show the Analogy that exists between them.*

XX. is Mr. Faraday's paper on new Compounds of Carbon and Hydrogen, already reprinted in the *Annals* for January and February of the present year; and the two following articles on magnetism developed by rotation, by Messrs. Babbage, Herschel, and Christie, we omit noticing in this place, for the reasons already stated.

XXIII. *On the Annual Variations of some of the principal Fixed Stars.* By J. Pond, FRS. Ast. Roy.

XXIV. *On the Nature of the Function expressive of the Law of human Mortality, and on a new Mode of determining the Value of Life Contingencies.* In a letter to F. Baily, Esq. FRS. by Benjamin Gompertz, Esq. FRS. E. W. B.

2. *Memoirs of the Astronomical Society of London, Vol. II. Part I.*

The First Part of the Second Volume of the Memoirs of the Astronomical Society of London has just been published; and the following are its contents:—

On the Method of determining the Difference of Meridians, by the Culmination of the Moon. By Francis Baily, Esq.

On the Utility and probable Accuracy of the Method of determining the Sun's Parallax by Observations on the Planet Mars near his Opposition. By Henry Atkinson, Esq.

On the Corrections requisite for the Triangles which occur in Geodesic Operations. By Captain George Everest.

On the Rectification of the Equatorial Instrument. By Prof. Littrow.

On the Variation in the Mean Motion of the Comet of Encke, produced by the Resistance of an Ether. By M. Ottaviano Fabrizio Mossotti.

Observations of the Solstice in June, 1823, *made at Paramatta, New South Wales.* Communicated by Sir Thomas Brisbane.

Observations made in the Years 1823-4 *at Paramatta, New South Wales.* Communicated by Sir Thomas Brisbane.

On a new Instrument, called the Differential Sextant, for measuring small Differences of angular Distances. By Benjamin Gompertz, Esq. the Inventor.

Observations on some singular Appearances attending the Occultation of Jupiter and his Satellites on April 5, 1824. By Mr. Ramage, Capt. Ross, and Mr. Comfield.

Observations on the Occultation of the Herschel Planet on Aug. 6, 1824. By Capt. John Ross.

An Account of the Arrival and Erection of Fraunhofer's large Refracting Telescope at the Observatory of the Imperial University at Dorpat. By Prof. Struve.

On a new Zenith Micrometer. By Charles Babbage, Esq.

The Results of Computations on Astronomical Observations made at Paramatta, in New South Wales, under the Direction of Sir Thomas Brisbane, and the Application thereof to investigate the Exactness of Observations made in the Northern Hemisphere. By the Rev. John Brinkley.

A short Account of a new Instrument for measuring Vertical and Horizontal Angles. By George Dollond, Esq.

Observations made at Bushey Heath, from May 17, 1816, *to Dec.* 7, 1824. By Col. Beaufoy.

On Astronomical and other Refractions; with a connected Inquiry into the Law of Temperature in different Latitudes and at different Altitudes. By Henry Atkinson, Esq.

A Report on the Properties and Powers of a new 3-feet Altitude and Azimuth Circle, lately fixed at the Rectory-House of South Kilworth in the County of Leicester:—constructed by Edward Troughton, and divided by T. Jones. Drawn up by the Rev. William Pearson.

Observations made at Paramatta, in New South Wales, by Major-General Sir Thomas Brisbane. To which are annexed

Observations made by Mr. C. Rumker, at Stargard, New South Wales, on the Comet which appeared in July, 1824.

Observation of an Eclipse of the Moon, taken at Chouringhy, near Calcutta, in the Year 1798. By the late Col. R. H. Colebrooke.

Observations of the Eclipses of Jupiter's Satellites, taken at Chouringhy, in the Years 1797, 1798, 1799, 1800, 1801, *and* 1803. By the late Col. R. H. Colebrooke.

Observations of the Eclipses of Jupiter's Satellites, taken at Chouringhy, in the Years 1821, 1822, *and* 1823. By Captains Hodgson and Herbert.

Observations of the Occultations of the Pleiades by the Moon, in July and October, 1821. By the Rev. W. Pearson.

Report of the Council to the Fifth Annual General Meeting.

List of Presents, &c. &c.

With an Appendix, *On the Construction and Use of some new Tables for determining the apparent Places of nearly* 3000 *principal Fixed Stars.* Drawn up by Francis Baily, Esq.

3. *Avium Species Novæ quas in Itinere per Braziliam Annis* 1817—1820, *jussu et auspiciis Maximiliani Josephi I. Bavariæ Regis Suscepto, collegit et descripsit Dr. J. B. De Spix, Ordinis Regii Coronæ Bavaricæ Civis Eques, &c. &c. Tabulæ* 115 *a M. Schmidt, manacensi Depictæ. Monachii,* 1824.

It is with great pleasure we observe the rapidity with which the continental naturalists describe and even figure their discoveries; for in less than two years after the return of the travellers sent to collect the subjects of natural history in the forests of South America, by the King of Bavaria, Dr. Spix has been enabled, by the excellent state of the continental museums, to write monographs on the reptiles and birds which were found in his travels. In the part of the reptiles (the snakes) he was certainly assisted by Dr. Wagler, a most enthusiastic erpetologist, who is engaged on a history of this order of animals.

The work is published in a size worthy of the royal auspices, under which it is produced; for although it is not quite so large as the "large work on Egypt," the large paper copies are not much inferior. The figures are lithographic, and do great credit to the talent of the artist; for the general accuracy of their outline and the minuteness of their detail, and indeed as works of art, they merit some notice, as the part at present under consideration presents in the three first plates two distinct kinds of lithography, and a kind of mixture between the two. For the King of the Vultures (Cathartes Papa) is a specimen of the usual chalk-like style of lithography; while the Rat-eating Eagle (Aquila Urubi-

tinga, t. 64), t. 1, b. and the Kitelike eagle (Aquila Milvoides), a kind of illustration of the engraving which we have never before observed in natural history plates. The best idea of it will be conveyed by considering it as an exact lithographic representation of an excellent wood-cut; and every one who has seen the works of Bewick must be ready to allow that this kind of engraving is well adapted for representing this order of animals. It appears to be done by covering the stone with ink in the form of the bird, and then scraping off those parts which are wanted to be light, and it leaves the outline sharp and distinct, which is not the case with lithography in general. Mr. Schmidt has only used this kind of lithography for the dark parts of birds, although there appears no reason why it should not, like wood-cuts, equally represent the more delicate tints; he has often used it in conjunction with the other kind, as may be observed in the Caracara (t. 1, a). The Tezoura, t. 8, b.

The present volume only contains the Raptorial and part of the Passerine birds. Dr. Spix divides the birds into natural groups without taking any notice of the orders; and under the families he gives an abridged account of the habits of the birds which are referable to them, which is the only notice that he takes of their manners, except in a very few instances.

The first family he calls *Falcones*, which appears to comprehend the Falcons and Vultures of Linnæus. In this family, he describes three new genera, as follows:

1. GYMNOPS. "Rostro subelongato gallinaceo, haud alto, subcylindrico, circa basin, præcipue ante oculos nudo, apice subadunco, oblique descendente; naribus rotundis; collo exserto; tarsis breviusculis; alis cruciatis; cauda æquali vix brevioribus; unguibus compressioribus, gracilioribus."

This genus is very nearly allied to the *Polybori* of Vieillot, which M. Spix also adopts: it contains four species, amongst which are the Falco formosus of *Latham*, the type of Vieillot's genus *Ibycter*, and *Falco Ater*, the type of the genus *Daptrius* of the latter.

2. MILVAGO. Habitu milvo, rostro *Gymnopi* simillimo, breviore, acuto, minus adunco, et circa oculos multo minus nudo; alis longioribus, caudam breviorem, æquantibus; naribus rotundis; gula non nuda.

This genus is also very nearly allied to Polyborus, and is, perhaps, the *Daptrius* of Vieillot; but in fact Cuvier appears to be right when he refers them all to *Caracara:* it only contains one species, *M. ochrocephalus*, which is in the British Museum.

3. BIDENS. Tinnunculoides, exiguus; pedibus gracilioribus, rostro brevi subcompresso plus minusve, bidentato; alis cruciatis, cauda brevioribus.

This genus is here first separated from the falcons, to which it is considerably allied, but the name was long ago employed by

Linnæus for a genus of plants; and Mr. Vigors, in his excellent paper on the Falconidæ, in the Zoological Journal, has lately, not knowing that Dr. Spix had separated it, formed it into a genus under the name of *Harpagus,* which must, therefore, be retained. He describes six species, two of them figured by Temminck, and he considers what the latter called the young of F. bidentatus (t. 228), as a distinct species, under the name of *Albiventer,* which must, therefore, be H. albiventer; he adds to this genus the Falcons, F. Sparverius (*Buff.* t. 465), F. Dominicensis (*Buff.* t. 444), and F. aurantius of *Latham;* but these birds, having reticulated legs, do not belong to Mr. Vigors' genus, but are true falcons. Dr. Spix has described the male and female of the first of these birds; and he says, the second is not the female of the first, as is generally supposed.

Unfortunately the genera are not very accurately defined, nor is the character of them or of the species expressed according to the usual rules of natural history, and the synonima are limited.

In the second family of *Striges,* there is only one genus, containing six species, five of which appear to be hitherto undescribed.

The third family or *Psittaci,* are divided into genera which appear to be analogous to the subgenera of Kuhl. They are,

1. Arara, the *Ara* of authors, containing eight species.

2. Aratinga characterized thus; Macrurus, minor modax; orbitis nudiusculis; genis angustis, vix vel raro nudis; rostro dentato, graciliore; cauda longiuscula cuneata; rectricibus intermediis longioribus. This genus contains several of the *Psittacara* of Mr. Vigors, but is not confined within the same limits as that genus; it contains 18 species, amongst which are two that Mr. Vigors has lately described as species of Psittacara. Dr. Spix figures, t. 11, a species of parrot, which he has dedicated to the King of Bavaria under the new generic name of *Anodorhynchus,* but he has not given the characters of it. The bird is peculiar, as the name partly implies, for the upper mandible is very long and much arched, and destitute of any notch in its lower edge. In this family there are also described four love-parrots, under the generic name of *Psittaculus,* and 17 species of true parrots.

The family of *Cuculi,* which Dr. Spix describes in the following manner, may be given as a specimen of the usual way in which these groups are characterized.

Insectivori, taciturni, non scansorii, sed sedentarii. Strigum modo breviterque volantes; digitis frequenter gracillimis, 2 anticis, 2 posticis; rostro arcuato; colla, alis, et tarsis abbreviatis.

In the genus *Trogon,* there are described nine species, some of which are exceedingly beautiful, though not so brilliant as those of the southern part of the Old World. Dr. Spix also describes

five species of *Bucco;* the whole of them properly belong to the genus *Capito* of Temminck, as that author has restricted the former genus to those species which are only found in the Old World. A bird very nearly allied to the latter genus is described under the name of Cyphos Macrodactylus, and this family, a new genus, is also established under the often-used name of

MACROPUS. Cuculinus, tarsis elevatis; cauda elongata, dependente, gradata, inæquali; alis breviusculis, rostro trigono peracuto edentato; naribus lateralibus oblongis, collo non abbreviato; digitis fissis.

Two species are described as belonging to this genus, the name of which must be altered. Of the genus Galbula, three species are described, one under the name of G. tridactilis; but we are not certain that it is the same bird as that which Vieillot has described under that name ; but it evidently is the same as the G. Ceycoides of Mr. Such.

In the *Pici,* consisting only of the genus *Picus* of Linnæus, 16 species are described, amongst which is one under the name of *P. campestris,* t. 46, which is peculiar for being gregarious, and not climbing trees, but living in the fields, and picking the insects from the mule's dung.

The next family of *Picæ,* a new species of *Coracina,* is described under the name of *C. ornata,* a new *Prionites, P. Martii,* t. 60; but the most interesting novelties of this family are the addition of three new *Cassici* and six *Icteri,* amongst which the figures of these birds, those of plate 64, illustrate excellently the superiority of the style of engraving, to which I have before referred, for ornithological subjects.

In the *Turdi,* or thrushes, M. Spix describes five species of the typical genus, three species of *Myiothera,* and a new genus, with the following characters, under the name of

PHILLYDOR. Insectivorus, ad ripam aquarum solitaria ambulans; cauda longiuscula, inæquali Dendrocolaptum modo sed molli; crista capitis brevi, plicatili; rostro subulato, lateraliter subcompresso, ad apicem subcylindrico, subdeclivi, subadunco, emarginato; maxilla inferiore breviore quam superiore; naribus subbasalibus, rotundis, minutis; rictu oris usque infra oculos elongato; tarsis breviusculis.

Three species are described and figured, all of which appear to be very nearly allied to the genus *Anabates* of Temminck, which the author refers to his family *Dendrocolaptes,* and describes three species. Two species of the genus Anthus are described, one belonging to Mr. Vigors' new genus *Corydalla,* the other a true Anthus.

This family ends with a new genus under the name of

FIGULUS. Ad marginem aquarum ac sylvarum solitaria et quasi domestica ambulans, nidum fornicis instar e limo supra, arbores non altas inter ramos construens, cauda æquali brevi-

uscula; rostro longiusculo subulato, ad apicem subarcuato non emarginato, rictu oris usque infra oculos fere prolongato, subarcuato, naribus subbasalibus ovatis, vix tectis.

Only one species of this genus is described and figured.

In the family *Certhiadæ*, several new genera are described.

1. Campylorhynchus. Turdinus; rostro longiusculo arcuato, crassiusculo, ad basin latiusculo, versus apicem cylindrico; naribus ovatis subbasalibus, non obtectis; digito postico, reliquis crassiore; alis breviusculis; rictu oris usque ad oculos elongato.

Two species are referred to this genus, *C. scolopaceus* and *C. striolatus.*

The genus Trochilus is restricted, and two new genera are separated from it.

1. Gryphus. Magnus; rostro trigono, rectiusculo versus apicem lateraliter serrato, compresso, ad apicem adunco, maxilla inferiore ad apicem ascendente; cauda latiuscula, æquali non elongata. Only one species is described and figured. G. ruficollis, which is T. Nævius, *Pl. Col.* t. 120, f. 3.

2. Colibri. Rostro crassiusculo, versus apicem intumido, subhiante, maxilla inferiore ad apicem, subascendente.

The name of this genus appears to be taken from the French name of the humming birds in general; it contains six species, but the genus still requires division.

In the last family of *Dendrocolaptes,* there are two new genera, under the names of,

1. Sphænura. Rostro longiore subulato, vix arcuato, subcylindrico, ad apicem obtuso; naribus rotundioribus, digitis ad basin connatis; cauda molliore detrita; digitis anticis lateralibus inæqualibus. This genus, according to the plates, contains two species; but in the description, one is referred to the genus *Anabates;* it is distinct from *A. striolatus* of Temminck. Of the latter genus there are three species, and of *Synallaxis* one species described. To this family is added a new genus separated from Parus under the name of

2. *Parulus.* Paro similis; alis brevissimis; cauda longiore gradata; rostro tenui brevi ad apicem subarcuato; maxilla inferiore compressa, apice ascendente; naribus longitudinalibus membrano obtectis. One species is described, and the P. biarmicus, of *Linnæus,* appears to be referrible to the same genus. Of the genus *Dendrocolaptes,* twelve species are described, several of which are new.

We wait with impatience for the remaining parts of this interesting work, which cannot fail to be very beneficial to the progress of science. It is only to be regretted that more is not said respecting the habits of these birds, for if the travellers who have observed them in their native places neglect to do so, from whom can we expect it; that there are not more numerous

references to other authors, for surely many of these birds must have been described by *Azara*, and named by Vieillot; and that the generic characters are not more distinctly defined. However, with these faults, which may be corrected as the work continues, we strongly recommend it to the public libraries of this kingdom. J. E. G.

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 23 (*continued*).—Edmund Davy, Esq. lately elected, being unable to attend for admission, requested that his name might be inserted in the printed lists of the Society, which was granted accordingly. The following is an abstract of Mr. Dalton's paper on the Constitution of the Atmosphere, read at this meeting.

After some preliminary remarks, the author observes, that whatever may be thought of Newton's hypothesis as to elastic fluids, as far as the *mechanical* effects of such fluids are objects of inquiry, we may safely adopt it; namely, that *each fluid is constituted of particles repelling one another by forces inversely as their central distances*, at least within ordinary limits of condensation and rarefaction.

After adverting to the fact that mixtures of various elastic fluids, such as is the atmosphere, composed of atoms of different volumes and elasticities, do notwithstanding observe the same laws of condensation and rarefaction as simple elastic fluids, and to the difficulties which this fact throws in the way of the Newtonian hypothesis, Mr. D. puts a case which he thinks has not before been considered, and which may assist us materially in forming a correct notion of such mixed atmospheres.

Two equal cylindrical pipes are conceived to be placed perpendicular to the horizon, in contact, and of indefinite length, close at the bottom, and open at the top. These are supposed to be filled with two gases of different kinds, the one with carbonic acid, and the other with hydrogen, in order to show the contrast more strikingly. The columns of gases are assumed each to be of the weight of 30 inches of mercury, and consequently will represent vertical columns of atmospheres of the respective gases equal in weight to like columns of the earth's atmosphere. Mr. D. calculates from known principles that the column of carbonic acid gas will terminate at 30 or 40 miles of elevation, or at least will become of such tenuity as that it may be disregarded. In like manner that of hydrogen will be found to become insignificant above 1200 miles of altitude. The author then supposes

that horizontal air-tight partitions are made across both tubes at any given intervals of distance, and that openings are made, so that the gases in the corresponding horizontal cells may communicate with each other, in which case each gas, as is well known, would divide itself equally between the two cells. For 30 or 40 miles both gases would be found in each cell; but for the rest of the column, namely, for 1000 miles or upwards, there would be nothing but hydrogen in both cells.

In the next place, Mr. D. conceives the horizontal partitions to be withdrawn, and considers what change would ensue. There would have been many cells about the summit of the carbonic acid atmosphere which, when opened for the purpose of communication, would part with half their contents to the collateral cells, but *half* the contents would not be able to fill the *whole space* of the cell, by reason that the gas was at its minimum density before. Hence the gas would be confined to the lower half of the cells, and there would be no carbonic acid in the upper parts. Of course when the partitions were removed, the carbonic acid in each cell would descend till it came in contact with the like gas of the inferior cell. Thus there would be a slight descent of the upper regions of carbonic acid gas. The same also would happen to the hydrogen gas about the summit of its atmosphere, and a still more considerable descent would take place. Mr. D. seems to think there would be no material change in the mixed atmospheres afterwards. Thus the two mixed atmospheres would exhibit equal *volumes* of each gas in the lowest cells, or at the surface of the earth, though in the whole compound atmosphere the two gases are of equal *weights.*

All this would take place according to the author's arguments were the mixed atmospheres *quiescent;* but if the atmospheres are like the earth's atmosphere, in a constant state of commotion, greater or less, still there will be a constant tendency towards that state of equilibrium which is above described. In the conclusion Mr. D. states, that he has a series of observations which support the opinion that the atmosphere at different seasons and elevations exhibits different proportions of its elements in association, which he intends to bring forward on some future occasion.

March 2.—The Hon. and Rev. Richard Carlton, and Lieut.-Gen. Sir Rufan Shaw Donkin, were admitted Fellows of the Society.

Two papers, by Sir E. Home, Bart. VPRS. were read, on the Coagulation of Blood in an Aneurismal Sac, and also of Blood taken from the Arm, by heated Iron. In the first paper, the author describes the symptoms of the disease in the patient on whom the experiment was tried, during and subsequently to its performance, which was effected by means of a heated acupunctorial needle; together with the state and characters of the blood

so coagulated. To the second paper, in which the effects on the blood of heated bodies at various temperatures are described, is annexed an account by Mr. Brande, of the chemical changes they produce in it.

March 9.—A paper was read, on the Analysis of Oil of Wine, with Remarks on the Salts called Sulphovinates; by Mr. H. Hennell, of Apothecaries' Hall: communicated by W. T. Brande, Esq. Sec. RS. The following is an abstract of this paper.

Mr. Hennell at first supposing that the elements of oil of wine were the same as those of sulphuric ether, endeavoured accordingly to determine their relative proportions in the former substance, by passing its vapour over ignited peroxide of copper. In this process, portions of sulphurous acid gas and sulphate of copper were invariably obtained; in attempting to ascertain the origin of which, the oil of wine was heated in solution of muriate of barytes, but no precipitate or even cloudiness was produced in it, though litmus paper at the same time indicated the presence of free acid. On concentrating the solution, however, a precipitate of sulphate of barytes was gradually formed; showing that either the sulphuric acid was in some state of combination interfering with its action upon tests, or that its elements existed in the oil of wine in some unusual state of arrangement. From 200 grains of pure oil of wine, treated with solution of potash, evaporated to dryness, and ignited, and then treated successively with nitric acid and muriate of barytes, were obtained 218·3 of sulphate of barytes, indicating 74 of sulphuric acid.

On resuming the analysis with peroxide of copper, with due care, and the additional precautions suggested by the nature of the substance as just pointed out, it appeared that 100 grains of oil of wine contain 53·70 of carbon, and 8·30 of hydrogen: the deficiency = 38 parts being referable to the sulphuric acid, as shown by the experiments above-mentioned. These proportions indicate the hydrocarbon combined with the sulphuric acid to contain an atom of each constituent; but they do not show the quantity of hydrocarbon combined with the sulphuric acid, for oil of wine always holds in solution an excess of this hydrocarbon, from which it is impossible to free it. In order to determine, therefore, the quantity of hydrocarbon in *combination* with the sulphuric acid, some oil of wine was heated with water, and precipitated carbonate of barytes was then added to it, which was dissolved, with effervescence. When, however, the solution was evaporated, it soon became acid, and sulphate of barytes precipitated. On treating a further quantity of oil of wine in the same manner, but precipitating the barytic solution by carbonate of potash, and evaporating at a temperature of 150° Fahr. it yielded tabular crystals, not unlike chlorate of potash, very soluble in water and alcohol, and burning with a flame resembling that of ether. These crystals were found to contain, in 100 parts,

Potash	28·84
Sulphuric acid	48·84
Carbon	13·98
Hydrogen	2·34
Water.	7·00
	101·00

It thus appears, that in this salt four proportionals of carbon united with four of hydrogen, are combined with one of sulphuric acid, forming oil of wine.

Mr. Hennel ascertained that this salt was identical with that called sulphovinate of potash; and whilst preparing some of the sulphovinates, for the purpose of comparing them with the salts obtained from oil of wine in this manner, he found that a great reduction of the saturating power of sulphuric acid was produced by its mixture with alcohol; 440 grs. of acid mixed with an equal weight of alcohol, requiring for their saturation only 398 grs. of partially dried carbonate of soda, whilst an equal weight of pure acid required 555 grains of the same carbonate. This fact shows that sulphuric acid, by mixture with alcohol, is immediately converted into sulphovinic acid; and, in conjunction with the facts detailed in the former part of the paper, it also evinces that the loss of saturating power cannot be owing, as MM. Vogel and Gay-Lussac have supposed, to the formation of hyposulphuric acid.

By heating oil of wine either in solution of potash, or in water, much of the excess of hydrocarbon which it contains is liberated in the form of an oil, resembling in appearance some of the balsams. This oil, as well as the crystals which form spontaneously in oil of wine, yielded by analysis carbon and hydrogen, in proportions nearly approximating to those of olefiant gas; but in the analyses, which were several times repeated, a slight loss was always experienced, the cause of which Mr. Hennell was unable to ascertain.

A paper on the Mathematical Principles of Suspension Bridges, by Davies Gilbert, Esq. MP. VPRS. was also read; and the reading of a paper by Mr. Herschel, on a new Method of ascertaining the Parallax of the Fixed Stars, was commenced.

March 16.—N. A. Vigors, Esq. and —— Pearson, Esq. were admitted Fellows, and the name of —— Hawkins, Esq. was ordered to be inserted in the printed lists of the Society. The reading of Mr. Herschel's paper, of which we shall probably give an account in our next, was resumed and concluded.

A paper was also read, On the Expression of the Parts of Machinery by Signs; by C. Babbage, Esq. FRS.

In contriving his calculating engine,* Mr. Babbage found

* See *Annals*, N. S. vol. iv.

great difficulty from not having any regular method, by which he could find, at an instant's notice, the precise time at which any given piece began to move, and also the state of motion or rest, at the same instant, of all the other parts. He therefore devised a method of expressing all the motions of any machine, however complicated, by signs. This it is almost impossible to describe without figures; but the following statement of the information which may be derived, almost at a glance of the eye, from the paper on which the " mechanical notation " of any machine is expressed, will serve to show the important purposes to which the method may be applied.

1. The name of each part is written at length, and there are references from the name to all the drawings.

2. The number of teeth on each wheel, pinion, rack, or sector, is seen.

3. Any given part, a wheel for example, being named, it will be seen what immediately moves it, what drives the mover, and so on up to the origin of motion: and not only will the whole succession of movements be visible, but the manner in which they act; as, for instance, whether by being permanently connected, or in the manner of a pinion driving a wheel, or by stiff friction, or at intervals only.

4. The angular velocity of each part will be seen.

5. The comparative angular velocity, or the mean velocity.

6. All those parts which require adjustment will appear; and the order in which those adjustments should be made is pointed out.

7. At any part of the cycle of the engine's motion, it will be seen at a glance what parts are moving, what are at rest; and it will appear in what direction the motions of the moving parts take place, and whether their velocity is uniform or variable. It will also be seen whether any given bolt or click is locked or not.

8. Any part being named, the entire succession of its motions and intervals of rest is at once presented to the eye; and if the contemporary movements at any particular time be required, they will be visible adjacent to it.

Mr. Babbage gives, as specimens of his method, the mechanical notation of the common eight-day clock, and of the hydraulic ram.

In consequence of the approaching fast and festival, the Society then adjourned over two Thursdays, to meet again on April 6.

PROCEEDINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN, AT THE FRIDAY-EVENING MEETINGS.

Feb. 3.—The results of a chemical examination of caoutchouc and the properties of the pure substance, were stated from the

lecture table by Mr. Faraday. Numerous specimens of the substance in all states, and as applied to manufactures in the construction of vessels and clothing, were exhibited, having been furnished by Mr. Hancock.

Feb. 10.—Mr. Faraday gave an account of the results which Mr. Brunel has obtained, in his endeavours to apply the liquids resulting from condensed gases to the construction of powerful mechanical engines. It appeared that after having successfully encountered many difficulties, Mr. Brunel had succeeded perfectly in confining and manipulating with the substances, and that he found the data furnished by his experiments highly favourable. He is at present engaged in constructing a working machine.

A plant of Ficus Elastica stood in the library.

Feb. 17.—Mr. Griffiths's experiment on the existence of free alkali in glass was exhibited in the library.—Mr. Varley illustrated and explained the construction and use of his single microscope.—Mr. Brant produced an extraordinarily large bar of palladium, weighing above 6 lb. which came from Brazil.—A series of geological and mineralogical specimens from South America was also exhibited.

Feb. 24.—Mr. Cornelius Varley explained the construction and uses of his graphic telescope, an instrument intended to perform the office of the camera obscura of Dr. Wollaston, but admitting of the application at the same time of magnifying powers.

March 3.—The chemico-mechanical principles of the art of lithography were explained by Mr. Faraday, and illustrated by many of the actual operations on stone. The stones and drawings had been furnished by Mr. Hullmandel, whose superintendent assisted also in the operations. Numerous very fine specimens of the art lay upon the tables.

March 10.—Mr. Brande gave an experimental illustration of the state and quantity of alcohol in wines, and the manner in which its properties are diminished, and partly masked, by combination with the other principles present. Unadulterated port wines were selected for the purpose, and the proportion of alcohol shown to be very low as compared with common port wines. The various precautions required in the accurate analysis of wines were pointed out, and applied in the operation.

LINNEAN SOCIETY.

Jan. 17.—A paper was read, On some Cornish Species of the Genus *Labrus;* by Mr. Jonathan Couch, FLS. Among other species noticed in this communication were *Labrus Julis; Tinca* (Common Wrasse); *cornubiensis* (Goldsinny); *microstoma* (Corkwring); *trimaculatus;* and *Comber:* also *Perca inermis.*

ASTRONOMICAL SOCIETY.

Feb. 10.—The Sixth Annual General Meeting of the Society, was this day held at the Society's rooms in Lincoln's Inn Fields, for the purpose of receiving the Report of the Council upon the state of the Society's affairs, electing Officers for the ensuing year, &c.

The President, F. Baily, Esq. in the Chair.

From the Report, which was read by Dr. Gregory, we give the following extracts:

"In meeting the Astronomical Society of London at its Sixth Anniversary, the Council have great pleasure in being enabled still to use the language of cordial congratulation; for not only does the number of the Members and Associates of the Society continue to increase, and its affairs to prosper; but also the theory and practice of Astronomy (the extension of which was the sole object of the Society) have both been obviously promoted by the zeal and talent of many of its members and friends."

The Report proceeds to state, that "in 1822, the members and associates amounted to 188; in 1823, to 207; in 1824, to 210; in 1825, to 224; in February, 1826, to 237;—a number, in which are included several of the most eminent promoters of astronomy, not only in Britain but in Europe.

"Amongst the few members of whom the Society has been deprived by death, the Council think it proper to call your attention to the loss of Mr. Cary. As an artist of considerable eminence and high reputation, he was well known in the scientific world. Amongst the many excellent instruments which he contrived and perfected, he was the maker of the $2\frac{1}{2}$-feet Altitude and Azimuth Instrument at Konigsberg, with which M. Bessel made his first observations at that celebrated Observatory.

"Among the duties which it has devolved upon your Council to discharge, one of the most interesting has been the selection of papers (read at the ordinary meetings) for publication in the volumes of the Memoirs of the Society. The Second Part of the First Volume, which was nearly ready for delivery at the Anniversary Meeting of 1825, was shortly afterwards laid before the public, and has been well received by astronomers.—The First Part of the Second Volume is now nearly ready for publication; and the Council trust that it will experience an equally favourable reception. Besides several valuable papers tending to improve the theory of astronomy and of astronomical instruments, and others describing instruments which are entirely new; the several parts here alluded to contain tables which tend very much to facilitate the labours of the practical astronomer. Thus the second part of Vol. I. terminates with subsidiary tables

for facilitating the computation of annual tables of the apparent places of 46 principal fixed stars, computed by order of the Council; to which is prefixed a statement by the Foreign Secretary of the formulæ employed, and the elements adopted in their construction. These tables, with their introduction, occupy 76 pages.

"The tables of precession, aberration, and nutation, serving to determine the apparent places of about 3000 principal fixed stars, to which allusion was made in the last Report of the Council, have been completed to 180° of AR, and written out for the press. The remainder are in a state of considerable forwardness. These tables, together with an ample introductory paper on their construction and use, by the President of this Society, will constitute an appendix to the second volume of the Memoirs.

"Amongst the numerous communications which have been made from the associates of this Society, the Council may specify a very interesting and elaborate paper, forwarded to the Foreign Secretary by M. Plana, on some important inquiries in physical astronomy, which will be found in the second part of the second volume. The President also has received a letter from M. Bessel, requesting to know whether the Astronomical Society would patronize and promote a plan, which he had suggested, for making detached charts of the heavens. The President was requested by the Council to assure M. Bessel that the Astronomical Society would doubtless promote so laudable and useful a measure, as much as lay in their power. That active and indefatigable astronomer, pursuant to his general plan, now regularly observes all the smaller stars in zones, agreeably to the method suggested and practised by the late Rev. F. Wollaston. He has already completed the zones within 15° on each side of the equator; and in that space has observed upwards of 30,000 stars. The observations are annually published by M. Bessel, with the other observations made at the Royal Observatory at Konigsberg. When they are reduced (as there is great reason to hope they will be), they will constitute a most valuable accession to the stores of astronomy.

"Others of the associates have especially distinguished themselves, and have forwarded to this Society some very interesting communications, as the successive parts and volumes of the Memoirs will evince. In alluding to these distinguished characters, your Council cannot avoid noticing the indefatigable labours of M. Schumacher, Professor of Astronomy at Copenhagen. His *Astronomische Nachrichten*, or Astronomical Newspaper, has considerably facilitated the intercourse between astronomers in every part of the world; serving to record the observations of various interesting phænomena, as well as to draw the attention of observers to other phænomena about to appear. He has also published several compendious collections

of tables of great practical utility. Among these, your Council cannot omit a particular reference to the very important tables, which constitute the second part of his *Sammlung von Hülfstafeln,* and which have been prepared for the purpose of reducing the 50,000 stars contained in Lalande's *Histoire Céleste;* serving indeed to effect the reduction of any one of those stars in the short space of two or three minutes.

"Thus whilst M. Schumacher has laid all astronomers under considerable obligations by the publication of these tables, he has conferred a peculiar mark of his esteem upon the body now assembled, by dedicating this volume to the Astronomical Society; a distinction which they who know the talent and zeal of this our eminent associate, will be able to appreciate in an adequate manner.

"One of our associates, M. Struve, has devoted himself with great perseverance and success to the observation, and classification, of double stars; an important department of astronomical research, which was originally opened and pursued with his wonted assiduity and accuracy by our late revered President, Sir William Herschel.

"This subject has been still more extensively pursued, and with considerable ardour and zeal, by two of our members, Messrs. Herschel and South; whose labours on this very interesting branch of the science are contained in a paper read before the Royal Society, and which in itself forms the third part of the Philosophical Transactions for the Year 1824. Whoever has read that paper with attention must be struck with the vast labour and perseverance, the great accuracy and uniformity of result, with which those delicate observations have been made. Such an immense mass of interesting facts cannot fail to open new views to the contemplative philosopher, and extend our knowledge of the true system of the universe: and Mr. Herschel himself, in a communication about to be laid before the Royal Society, has made a happy application thereof, as explanatory of some of the phænomena connected with parallax. The indefatigable ardour of Mr. South in the cause of astronomy, induced him to follow up his researches on the same subject whilst he was in France; and he has recently made a communication to the Royal Society, of some new observations, of equal, if not superior, importance; and which will appear in a subsequent volume of the Philosophical Transactions.

"For these laborious and valuable researches and observations relative to double stars, the Council have awarded to each of those distinguished members and associate, Mr. Herschel, Mr. South, and M. Struve, the Gold Medal of the Society, which will be presented to them at a General Meeting expressly called for that purpose, as soon as the medals can be prepared.

"Sir Thomas Brisbane, Governor of New South Wales, has

devoted himself indefatigably to the practice of astronomy, at Paramatta in that colony, having taken out with him some excellent instruments for that purpose. He and his assistants have already made several thousand observations, the records of which have been sent over to this country: and it is hoped that they will be published, either in their original shape, or after they have been reduced to some appropriate epoch. Dr. Brinkley, of Dublin, one of the Vice-Presidents of this Society, has instituted a series of computations on Sir Thomas Brisbane's Observations, with a view to the comparison of the results thus furnished, with the results deduced from observations made in the northern hemisphere. This particular inquiry has served to confirm the accuracy of the constant of refraction, formerly exhibited by that illustrious astronomer in his well-known formula for that species of reduction. Dr. Brinkley's paper on this subject is printed, and will appear in Part I. Vol. II. of the Memoirs of this Society.

"Another of the members of the Astronomical Society, the Rev. Fearon Fallows, Astronomer at the Cape of Good Hope, has also made a great number of observations of the southern stars; and the Royal Society has published his Approximate Catalogue of 273 of the principal stars observed by La Caille.

"The continuance of observations, such as these, at two Observatories in the southern hemisphere, cannot but be productive of considerable benefit to the science of astronomy. In order, however, that they may be rendered subservient, in the highest degree, to the extension of this branch of knowledge, it is especially desirable that some efficient plan of co-operation should be arranged between the astronomers at some of the northern observatories, and those who are employed at the two above-mentioned stations, south of the equator. Those who are conversant with the history of astronomy will recollect that when La Caille went to the Cape of Good Hope, in 1751, he addressed a circular letter to the principal astronomers in Europe, enforcing the advantages of co-operation; and Lalande was in consequence sent to Berlin, to act in concert with him. Circumstances are now still more favourable for the production of advantageous results, provided a judicious plan of mutual co-operation be agreed upon. For while there is the Observatory established by Sir T. Brisbane in New South Wales, and that occupied by Mr. Fallows at the Cape; there are also in the northern hemisphere, M. Bessel at Konigsberg, M. Struve at Dorpat, and M. Argelander at Abö (the meridians of the four latter-mentioned places differing from each other but a very few degrees),—the respective astronomers, men of considerable science, activity and perseverance, and possessing instruments far superior to those which were in existence in the time of La Caille. The advantages of this kind of pre-arranged co-

operation, to which your Council here advert, are so well understood in the present advanced state of astronomy, that a mere hint will (it is hoped) suffice, to produce the desired concert."

The Report then adverts to the contributions and exertions of other scientific bodies, alluding to the erection of an Observatory at the University of Cambridge, and the still more recent announcement of a prize of 75*l.* at Edinburgh, to be awarded to the authors of the two best essays on Comets; also to the reduction of Mr. Groombridge's observations, and the arrangement and publication of those of Tobias Mayer, which have been determined upon by the British Board of Longitude. As another subject of congratulation, the Council mention the interest which appears recently to have been excited in the United States of America in the subject of astronomy.

"With respect to the Prize Questions proposed at the last General Meeting of the Society, the Council report that they have received only one answer to the first question, which, being just delivered in, is now under investigation. The period allotted for the determination of the second question will not expire till the next anniversary, and that allotted for the third question not till the anniversary in 1828: prior to which time the Council trust that the subjects proposed will have excited the attention of astronomers, and induced them to forward to the Society the result of their inquiries and investigations."

The Council then state their opinion, that it would tend materially to the advancement of astronomy, if an accurate description of every principal Observatory could be obtained, accompanied with a ground plan and elevation of the building; together with a description of the instruments employed, and drawings of such as are remarkable, either for their novelty or peculiar interest. "It is well known," they observe, "that there are several instruments in constant use on the Continent, and much approved by astronomers, which have not yet been seen in this country; and some in this country, which are not sufficiently known abroad; or even amongst ourselves. The Council would encourage every attempt to promote this species of information, by publishing in their Memoirs the accounts which they may from time to time receive on this subject, and the drawings with which they might be accompanied."

The Report concludes in the following manner:

"Your Council think it unnecessary to extend this Report to a greater length. It must be evident that many things which (as far as regard the objects and labours of this Society) were six years ago only matters of hope and anticipation, have now become subjects of mutual congratulation. But it can only be by a cordial and zealous co-operation of all its members, and by a continued course of perseverance, that the Society can ever

expect fully to attain the principal objects for which it was established; and which, as stated in their original address, are for the purpose of 'collecting, reducing, and publishing useful observations and tables:—for setting on foot a minute and systematic examination of the heavens:—for encouraging a general spirit of inquiry in practical astronomy:—for establishing communications with foreign observers:—for circulating notices of all remarkable phænomena about to happen:—for enabling the public to compare the merits of different artists, eminent in the construction of astronomical instruments:—for proposing Prizes for the improvement of particular departments, and bestowing medals or rewards on successful research in all:—and finally, for acting, as far as possible, in concert with every Institution both in England and abroad, whose objects have any thing in common with their own; but avoiding all interference with the objects and interests of established scientific bodies.' Keeping these objects in view, as constant landmarks, the Council trust that the Society will insure the approbation and applause of every friend of science; and that it will not only prove a source of interest and information to the members at large, but likewise tend to advance the progress of astronomy in every habitable and civilized part of the globe."

After reading the Report and the Treasurer's accounts, the members proceeded to ballot for the officers for the ensuing year, when the following were declared to have been duly elected:—

President.—Francis Baily, Esq. FRS. LS. and GS.

Vice-Presidents.—Rev. John Brinkley, DD. FRS. Pres. RIA. and And. Prof. Ast. Univ. of Dublin; Capt. F. Beaufort, RN. FRS.; Henry Thomas Colebrooke, Esq. FRSL. and E. FLS. and GS.; and Davies Gilbert, Esq. MP. VPRS. FLS. and GS.

Treasurer.—Rev. William Pearson, LL.D. FRS.

Secretaries.—Olinthus G. Gregory, LL.D. Prof. Math. Royal Mil. Acad. Woolwich; and Lieutenant William S. Stratford, RN.

Foreign Secretary.—J. F. W. Herschel, Esq. MA. Sec. RS. Lond. and FRSE.

Council.—Colonel Mark Beaufoy, FRS. and LS.; Benjamin Gompertz, Esq. FRS.; Stephen Groombridge, Esq. FRS.; James Horsburgh, Esq. FRS.; Daniel Moore, Esq. FRS. SA. LS. and GS.; John Pond, Esq. FRS. Ast. Royal; Edward Riddle, Esq.; Richard Sheepshanks, Esq. MA.; W. H. Fox Talbott, Esq. BA.; and Edward Troughton, Esq. FRSL. and E.

GEOLOGICAL SOCIETY.

Feb. 17.—At the Anniversary Meeting of the Society held this day, the following gentlemen were elected Officers and Council for the year ensuing:—

President.—John Bostock, MD. FRS.

Vice-Presidents.—Sir Alexander Crichton, MD. FRS. and LS. Hon. Memb. Imp. Acad. St. Petersburgh; Rev. W. D. Conybeare, FRS.; William Henry Fitton, MD. FRS.; and Charles Stokes, Esq. FRA. and LS.

Secretaries.—W. J. Broderip, Esq.; R. J. Murchison, Esq.; and Thomas Webster, Esq.

Foreign Secretary.—Henry Heuland, Esq.

Treasurer.—John Taylor, Esq.

Council.—Arthur Aikin, Esq. FLS.; Henry Thomas De la Beche, Esq. FRS. and LS.; J. E. Bicheno, Esq. Sec. LS.; Henry Thomas Colebrooke, Esq. FRSL. and E. FL. and Asiat. Soc.; Sir Charles Henry Colvil; George Bellas Greenough, Esq. FR. and LS.; Sir Charles Lemon, Bart. FRS.; Armand Levi, Esq.; Charles Lyell, Esq. FR. and LS.; William Hasledine Pepys, Esq. FRS. LS. and HS.; George Poulett Scrope, Esq.; J. F. Vandercom, Esq.; and Henry Warburton, Esq. FRS.

March 3.—The reading of Sir A. Crichton's paper, on the Taunus Mountains in Nassau, was concluded.

The great mountain groups forming the Taunus are portions of that vast chain which crosses the Rhine to Valenciennes; and in the duchy of Nassau they are composed of transition and trap rocks: they here separate into two ranges, nearly at right angles to each other. The southern chain lies on the north of Mayence and Frankfort, and its highest point is the Feldberg, 2600 feet above the level of the Mayne. The northern chain includes the Westervald, celebrated for its brown-coal. The strata of the southern face of the former chain consist of talc and quartz-slate dipping north-west; whilst those of the northern face are of grauwacke and clay-slate, inclining upward south-east. The summit is a decomposing quartz-rock, containing talc and iron, the sides and base of the mountain being formed of talc and slate. The baths of Schlangenbad are surrounded by slaty quartz: quartz-conglomerates occur near the foot of the southern chain; where also a thick bed of sandstone, resembling our new red sandstone, rests upon the calcareous deposits of the valley of the Mayne, quarries of which are seen at Wisbaden.

The valley of the Mayne, which is interposed between the northern and southern chains, is chiefly occupied by low hills of coarse shelly limestone, analogous to the upper freshwater formation of Paris, and quarries of it occur near Wisbaden and Hockheim: *Paludinæ* and *Modioli* abound in it. At Hockheim the beds are much dislocated, and at Wisbaden fossil bones are found, the teeth accompanying which refer them to animals allied to the *Lophiodon tapiroides*, and to the Sumatran Tapir. These calcareous deposits are only two hundred feet above the level of the Mayne, and they are perforated in many places by

basalt, upon which they rest. The basalt finally disappears south-east of Darmstadt, and is succeeded by primitive rocks. There are strong salt-springs at Soden, and various mineral waters near Frankfort and Hadnigstein.

The Falkenstein mountain, though composed of talc-slate, protrudes through the high table land in the form of basalt. To the north of this the older rocks disappear, and the district is occupied by grauwacke. The grauwacke is divided into quartzy grauwacke and grauwacke slate; the latter is very distinct from micaceous slate, and contains casts of *Spiriferi*, of the *Pleurobranchi* of Cuvier, &c.; the former offers encrinites, and unknown coralloids. The valley of the Lahn, between Coblentz and Diety, affords the best sections of grauwacke, and higher up that river the transition limestone appears at Baldowinstein. The schalstein (or problematic stone of Von Buch) is seen in all its varieties in the valley of the Aar, and with it are associated porphyry, carbonate of lime in veins, iron, and copper. At Diety and Baldowinstein, porphyry seems to rise through the limestone. Crystalline dolomite, resting upon transition limestone, is the most recent formation observable in the mountainous ranges of Nassau. No diluvial detritus is seen in any part of the duchy, but quartz pebbles in sand occur in the elevated plain between Selters and Nassau: these are supposed to have been torn from the grauwacke by local causes, and to have been deposited prior to the elevation of that formation. The author, reflecting upon the marine fossils on the summits of some of these mountains, infers, that the horizontal strata were formed at the bottom of a sea, and were subsequently elevated; and he is inclined to attribute the origin of the grauwacke to the attrition of the primitive rocks during the period of their elevation.

E. W. B.

Article XIII.

SCIENTIFIC NOTICES.

Chemistry.

1. *Vegetable Principle in Saponaria Officinalis.*

We are informed that Dr. Osborne of this city has detected a principle in the Saponaria officinalis, or Soap-wort, which in some of its characters more nearly resembles picrotoxia than any other proximate vegetable principle hitherto described, but is sufficiently distinguished from it in others. It was obtained from the decoction. It has an extremely bitter taste, is of a whitish colour, and crystallizes in prisms, both radiated and plumose. On the application of a low heat it fuses; and when the heat is increased, swells and blackens. It possesses neither

acid nor alkaline properties. Treated with sulphuric acid, and heated, it soon chars, and is entirely decomposed. It is soluble in ether and alcohol, and dissolves in less than twice its weight of cold water. It is insoluble in spirit of turpentine. After the plant had flowered, it unexpectedly ceased to afford any of this substance; so that it was necessary to postpone all further experiments on it until next season, when we hope to be enabled to lay a more detailed account of it before our readers.

In the course of those experiments, it appeared that the abstergent quality of the plant, which has been long observed, and from which its name is derived, was solely owing to a viscid gummy matter, which forms an emulsion when rubbed in contact with greasy particles; and which also produces the great quantity of froth which is formed on agitating the decoction.—(Dublin Philosophical Journal.)

2. *Phosphorescence of certain Fluids.*

The following fluids have been found by Dr. Brewster to be phosphorescent when poured into a cup of heated iron.

1. Albumen (white of an egg) diluted in water.
2. Isinglass, solution of.
3. The two preceding solutions mixed.
4. The saliva.
5. Soap and water.
6. Solution of rhubarb.
7. Solution of common salt.
8. Solution of nitre.
9. Tallow. The phosphorescence of tallow may be distinctly observed when a candle is put out in a dark room.
10. Alcohol.
11. Ether burns with a blue flame.
12. Oil of dill seed.
13. Oil of olives.
14. Solution of alum, very faint.

In making these experiments, Dr. Brewster found that alcohol *would not inflame* when poured upon a red-hot iron, while ether burned with great readiness.—(Edin. Journ. of Science.)

3. *On Rinmann's Green.*

This colour is an intimate mixture of the protoxide of cobalt and the oxide of zinc, which assumes a very lively green tint, after it has been heated to redness. In order to form this green suddenly, as if by the eruption of a volcano, mix together two parts of nitrate of zinc, and one part of the subacetate of cobalt, and expose the mixture to a spirit of wine lamp, in a glass globe with a short neck. This mixture soon becomes liquid, and appears at first of a rose-red colour, then purple, then blue. In an instant it flames, detonates, becomes dry, and assumes a

green colour. The product is scattered upon the vessel in the form of small rolled leaves of tea.—(Bull. des Scien. Nat.)

4. *Formula for the Preparation of the Sulphate of Rhubarb.* By Mr. George W. Carpenter.

Boil for half an hour 6 lb. of coarsely bruised Chinese rhubarb in six gallons of water, acidulated with two and a half fluid ounces of sulphuric acid; strain the decoction, and submit the residue to a second ebullition in a similar quantity of acidulated water; strain as before, and submit it again to a third ebullition, unite the three decoctions, and add by small portions recently powdered lime, constantly stirring it to facilitate its action on the acid decoction. When the decoction has become slightly alkaline, it deposits a red flocculent precipitate, which is to be separated by passing it through a linen cloth and dried, after which reduce it to powder, and digest in three gallons of alcohol at 36° in a water-bath for several hours, which dissolves the rhubarbine; separate this solution from the calcareous precipitate, distil off three-fourths of the alcohol; there then remains a strong solution of rhubarb, to which add as much sulphuric acid as will exactly neutralize it; evaporate this slowly to dryness, the residuum will be of a brownish-red colour, intermingled with brilliant specks possessing a pungent styptic taste, soluble in water, and its odour that of the native rhubarb.

This preparation is a concentrated form of the active principle of that valuable cathartic, separated from the ligneous and mucous portions, and bears the same relation to the crude substance that quinine does to Peruvian bark. It is well worthy the attention of physicians, as the quality of rhubarb is so various that the dose is very uncertain. This preparation will be of uniform strength, and may be administered safely to new-born infants.—(Silliman's Journal.)

Mineralogy.

5. *Optical Structure of Edingtonite.*

Mr. Haidinger, the discoverer of this new mineral, which he has described in our last number, and which has been analyzed by Dr. Edward Turner, was so good as to put into my hands three very minute crystals of it for the purpose of examination. It has one axis of double refraction coincident with the axis of the octohedron, which is its primitive form. The character of its action upon light is negative, like that of calcareous spar. This result affords another proof, if any were wanting, of the infallibility of the optical law of primitive forms. D. B.—(Edin. Jour. of Science.)

6. *New Analysis of Dioptase.*

The former analyses of Vauquelin and Lowitz, differ widely from the following results now obtained by Vauquelin.

	Lowitz.	Vauquelin. 1st analysis.	Vauquelin. 2d analysis.
Silex	33	28·57	43·181
Carbonic acid.		18·67	
Lime		24·18	
Ox. of copper.	55	28·58	45·455
Water.	12		11·364
	100	100·00	100·000

Vauquelin's second analysis was made up by decigrammes. —(Edin. Jour. of Science.)

Zoology.

7. *Fossil Deer of Ireland.**

Since the publication of our last number, a very fine and perfect skeleton of this animal has been placed in the Museum of the Royal Dublin Society. For this very desirable addition to zoology, we are indebted to the liberality and love of science of the Rev. Archdeacon Maunsell, and the skill and anatomical knowledge of Mr. Hart, Member of the College of Surgeons in this city, by whom a memoir on the subject has been published, from which we make the following extracts. Archdeacon Maunsell, in his communication to the Royal Dublin Society, gives the following account of the situation in which these bones were found:—

"The valley in which the remains were found contains about twenty plantation acres, and the soil consists of a stratum of peat about a foot thick, and immediately under this a stratum of shell marl, varying from $1\frac{1}{2}$ to $2\frac{1}{2}$ feet in thickness; in this many of the shells retain their original colour and figure, and are not marine; under the marl there is a bed of light blue clay, through this one of my workmen drove an iron rod, in several places, twelve feet deep, without meeting opposition. Most of the bones and heads, eight in number, were found in the marl; many of them, however, appeared to rest on the clay, and to be merely covered by the marl. The remains were disposed in such a manner as to prevent the possibility of ascertaining the exact component parts of each skeleton; in some places portions were found removed many yards from others, and in no instance were two bones found lying close to each other. Their position also was singular; in one place two heads were found, with the antlers entwined in each other, and immediately under them a large blade bone; in another, a very large head was discovered,

* See *Annals* for June, 1825.

and although a most diligent search was made, no part of the skeleton found; within some hundred yards, in another, the jaw-bones were found, and not the head. The conclusion which, I conceive, may fairly be deduced from such a position of the various parts of the animals is, that there must have been some powerful agent employed in dispersing them after their death; and as I consider it impossible that their own gravity could have been sufficient to sink them through the various strata, I conceive these must have originated subsequently to the dispersion of the bones. I also think, that if they had been exposed for any time to atmospheric influence, they never could have been preserved in their present extraordinary state of perfection.

"The hills immediately adjoining this valley are composed of limestone, with a covering of rich mould of various degrees of thickness. One of them, the base of which is about thirty acres, rises directly from the edge of the valley, with sides very precipitous, and in one place perfectly perpendicular, of naked limestone. In every part of this hill the superficies comprises as much stone as mould; on the side nearly opposite, the hill is equally high, but the sides not so steep, and the covering of mould thicker; on the other sides the ground only rises in some degree (twenty or thirty feet perhaps) and consists of a thin mould, and immediately under a *very hard* limestone gravel. Indeed, except where limestone forms the substratum, this is the character of all the soil in the vicinity except the Corkases, which are evidently alluvial. I am fully aware, that assuming the destruction of the animals to have been occasioned by a flood, they would naturally have retreated from the water to the hills, and that, as they probably met their fate there, their remains should have been discovered on the summit of the hills, and not in the valley, particularly as one of them is perfectly flat on the top, which contains six or seven acres. I apprehend that the remains of many of them were deposited on the tops of the hills, but as they have *now* only a slight covering of mould, not sufficient to cover a small dog, they were formerly perfectly bare; and as they were thus devoid of the means of protecting the remains from the atmosphere, whatever was left there soon became decomposed, and resolved into portions of the mould, which is now to be found on the hills. This remark I conceive also to be applicable to the soil with the substratum of limestone gravel, which affords quite as little material for preserving the bones as the hills do. It is material that I should observe that of eight heads, which we found, none were without antlers; the variety in character also was such as to induce me to imagine, that possibly the females were not devoid of these appendages; unfortunately, however, from the difficulty of raising them, being saturated with water, and as soft as wet brown paper, only three were at all perfect. Your's most truly,

"William W. Maunsell."

Mr. Hart's description is as follows:—"This magnificent skeleton is perfect in every single bone of the framework which contributes to form a part of its general outline: the spine, the chest, the pelvis, and the extremities, are all complete in this respect; and when surmounted by the head and beautifully expanded antlers, which extend out to a distance of nearly six feet on either side, forms a splendid display of the reliques of the former grandeur of the animal kingdom, and carries back the imagination to a period when whole herds of this noble animal wandered at large over the face of the country. To proceed with a description of the several parts of this specimen in detail, I shall commence with the horns, which give the animal its chief characteristic feature. *The horns.*—That the description of these may be the more intelligible, I will first explain the terms which I mean to apply to their several parts. Each horn consists of the socket or root, the burr or coronary circle, the beam or shaft, the palm and the antlers. The *socket* or root is the part of the horn which grows out of the frontal bone, and which is never shed; it is smooth, of a brown colour, an inch and half in length, and eleven inches three quarters in circumference; in the animal's life time it was covered by the skin. The coronary or bead-like circle, or burr, is a ring of small, hard, whitish prominences, resembling a string of pearls, which encircles the junction of the socket with the part of the horn which falls annually from the heads of all deer. The *beam* or *shaft* extends outwards with a curvature, the concavity of which looks downwards and backwards. This part is nearly cylindrical at its root, and its length equals about one-fourth of that of the whole horn; its outer end is spread out and flattened on its upper surface, and is continuous with the *palm*, which expands outwards in a fan-like form, the outer extremity of which measures two feet ten inches across, being its broadest part. Where the beam joins the palm, the horn undergoes a kind of twist, the effect of which on the palm is, to place its edges above and below, and its surfaces anterior and posterior; the anterior surface is convex, and looks outwards; the posterior is concave, and its surface looks towards that of the opposite palm. Such is the position of the horns, when the head is so placed that the zygomatic arch is parallel to the horizon, as it would be during progression, or whilst the animal stands in an easy posture. The *antlers* are the long pointed processes which project from the horns, two of which grow from the beam anteriorly; the first comes off immediately from the root, and is directed downwards, overhanging the orbit; this is called the brow antler, which in this specimen is divided into two points at its extremity.* The

* I have seen this antler divided into three points in two specimens, one at the Earl of Besborough's, county Kilkenny, (which measured eight feet four inches between the tips,) the other in the hall of the Museum of Trinity College: it is single in the greater number of specimens, as in those which Cuvier describes.

other antler, which comes off from the beam, we may call the sur-antler: in this specimen it consists of a broad plate or palm, concave on its upper surface, horizontal in its direction, and forked into two points anteriorly, an appearance which I have not observed in any other specimen of upwards of forty which I have seen, nor do I find it marked in any of the plates of those bones extant. There is one antler given off posteriorly from the junction of the beam with the palm; it runs directly backwards parallel to the corresponding one of the opposite horn. The inferior edge of the palm beyond this runs outwards and backwards: it is obtuse and thick, and its length is two feet six inches. From the anterior and external borders of each palm there come off six long pointed antlers. None of these are designated by any particular name. The number of the antlers of both sides taken together is twenty-two. The surface of the horns is of a lightish colour, resembling that of the marl in which they were found; they are rough, and marked with several arborescent grooves where the ramifications of the arteries by which they had been nourished during their growing state were lodged. The horns, with the head attached, weighed eighty-seven pounds avoirdupois. The distance between their extreme tips in a right line is nine feet two inches. *Head.*—The forehead is marked by a raised ridge extended between the roots of the horns; anterior to this, between the orbits and the root of the nose, the skull is flat; there is a depression on each side in front of the root of the horn and over the orbit capable of lodging the last joint of the thumb, at the bottom of which is the superciliary hole, large enough to give passage to an artery proportioned to the size of the horns. Inferior to the orbit we have the lachrymatory fossa, and the opening left by the deficiency of bone common to all deer, and remarkable for being smaller in this than in any other species. Below the orbits the skull grows suddenly narrower, and the upper parts of the nasal bones become contracted by a depression on either side, at the lower part of which is the *infra-orbitar* hole. The opening of the *nares* is oval, being five inches long by three broad, the greatest breadth being in the centre. From the roots of the horns to the occipital spine measures three inches and an half; the *occiput* descends at a right angle with this, being three inches deep to the foramen magnum: the greatest breadth of the *occiput* is eight inches. The temporal *fossæ* approach to within two inches of each other behind the horns. *Teeth.*—They do not differ from those of animals of the ruminating class. The incisors were not found, having dropped out; there is no mark of canine teeth; the molares are not much worn down, and are twenty-four in number. The skeleton measures, from the end of the nose to the tip of the tail, ten feet ten inches. The spine consists of twenty-six vertebræ, viz. seven cervical, thirteen dorsal, and six lumbar. The size of the cervical vertebræ greatly

exceeds that of the other classes, and the spines of the dorsal rise to a foot in height. The necessity of these bones being so marked is obvious, considering the strong cervical ligament, and powerful muscles, required for supporting and moving a head which, at a moderate calculation, must have sustained a weight of three quarters of a hundred of solid bony matter. The extremities are in proportion to the different parts of the trunk, and present a conformation favourable to a combination of great strength with fleetness. It is not the least remarkable circumstance connected with these bones, that they are in such a high state of preservation as to present all the lines and impressions of the parts which had been attached to them in the recent state. Indeed if we examine them as compared with the bones of an animal from which all the softer parts have been separated by maceration, the only perceptible differences in their physical properties are, that they are a little heavier, a degree harder, that their surface is brown, and that they all, with the exception of the horns, present a polished appearance, which is owing to the periosteum having been preserved, and still remaining to cover them as was discovered when they were chemically examined. The existence of fat or adipocire in the shaft of one of the bones mentioned by Archdeacon Maunsell, and which I saw in his possession, is a thing for which it is extremely difficult to account, as it occurred but in one solitary instance, and it did not appear that this bone was at all differently circumstanced from the rest. Those which I had an opportunity of examining, by boring holes in them, were hollow, and contained for the most part only a small quantity of black animal earth. I requested my friend Mr. W. Stokes to make an analysis of a small fragment of a rib, which he found to contain the following constituents:

Animal matter	42·87
Phosphates, with some fluates.	43·45
Carbonate of lime	9·14
Oxides	1·02
Silica	1·14
Water and loss	2·38
	100·00

With a view to ascertain the state of the animal matter, I had a portion of bone submitted to chemical examination by my friend Dr. Apjohn, at the laboratory of the New Medico-Chirurgical School, Park-street, of the result of which he gave me the subjoined statement:

"I regret that time did not permit my making a more particular examination of the Moose-Deer bone, which was left by you at my laboratory. Knowing that you were in possession of

a tolerably correct analysis of its earthy materials, my attention was directed to its animal constituents, which, as the following experiments establish, were found in a state of perfect preservation. The bone was subjected for two days to the action of dilute muriatic acid. When examined at the end of this period, it had become as flexible as a recent bone submitted to the action of the same solvent. The periosteum was in some parts puffed out by carbonic acid gas, disengaged from the bone, and appeared to be in a state of soundness. To a portion of the solution of the bone in the muriatic acid some infusion of galls was added, which caused a copious precipitate of a dun colour. This proved to be tannate of gelatine, mixed with a small portion of the tannate and gallate of iron. The cartilage and gelatine therefore, so far from being destroyed, had not been perceptibly altered by time."

Mr. Hart, in speaking of the specific character of the animal, makes the following observations: It is now clearly ascertained that the only large species of deer inhabiting the northern parts of America are the wapiti or Canadian stag (*Cervus Canadensis*), the rein-deer (*C. Tarandus*), and the moose or elk (*C. Alces*). The peculiar branching of the brow antlers of the rein-deer, and the rounded horns of the wapiti,* are characters sufficient to prevent us confounding either of these animals with the fossil species. The palmate form of the horns of the elk gave greater probability to the opinion of its specific identity with the fossil animal. A little attention, however, to a few circumstances, will show a most marked difference between them. First, as to size, the difference is very remarkable, it not being uncommon to find the fossil horns ten feet between the extreme tips, while the largest elk's horns never measure four feet. This measurement in a pair in the Museum of the Royal Dublin Society is three feet seven inches; the largest pair seen by Pennant in the house of the Hudson's Bay Company measured thirty-four inches.† The horn of the elk has two palms, a lesser one which grows forward from the front of the beam where the principal palm begins to expand. This is called brow antler by Cuvier, but it corresponds in situation rather to the sur-antler, there being, properly speaking, no brow antler attached to the root of the beam. The elk has no posterior antler similar to that of the fossil animal, nor does its beam take a similar arched direction, but runs more directly outwards.

Cuvier remarks, that the palm of the fossil horn increases in breadth as it extends outwardly, while that of the elk is broadest next the beam.

* A fine pair of this species, male and female, were exhibited by Mr. Bullock in this city a few summers ago. They did not answer to any description of Pennant or of Dr. Shaw, but had the characters of C. Canadensis as given by Cuvier.

† Pennant's Zoology, vol. i.

The palm of the elk's horn is directed more backwards, while the fossil one extends more in the lateral direction. The antlers of the elk are shorter and more numerous than those of the fossil.

As the horns of the fossil animal exceed in size those of the elk, so on the contrary does the skull of the latter exceed in size that of the former; the largest heads of the fossil species not exceeding one foot nine inches in length, while the head of the elk is frequently two feet. The fossil head is broader in proportion; its length being to its breadth as two to one; in the elk they are as three to one, according to Parkinson.* The breadth of the skull between the roots of the horns is but four inches in the fossil skulls; in that of the elk in the Society's Museum it is $6\frac{1}{2}$ inches.

Cuvier thinks it probable that the females of the fossil species had horns,† an opinion to which I am very much disposed to subscribe, from having observed that these parts present differences in size and strength, which appear not to be dependent on differences of age; for instance, the teeth of the specimen in Trinity College are much more worn down, and the sutures of the skull are more effaced than in the specimen described in this paper; yet the horns of the latter are much more concave, and more expanded than those of the former; and on comparing a single horn of each of these specimens together, that belonging to the Society exceeds the other by nearly a sixth in the length, and little less than a third in the breadth; it is not therefore unlikely that the animal whose horns were larger and more curved was a male. Something similar to this is observed in the rein-deer, both sexes of which have horns, but with this difference, that they are smaller and less branched in the female. Hence we find that this animal possessed characters of its own sufficient to prove it of a species as distinct from the moose or elk as this latter species is from the rein-deer or any other; therefore, it is improper to retain the name of elk or moose deer any longer; perhaps it might be better called the *Cervus megaceros*, a name merely expressive of the great size of its horns.

That this animal sheds its head furniture periodically is proved by the occasional occurrence of detached horns having the smooth convex surface below the burr, similar to what is observed on the cast horns of all deer. Specimens of this are to be seen in the Museum of Trinity College, and I possess one myself, of which I have had a drawing made. As every other species of deer shed their horns annually, there is no reason for supposing that that process occurred at longer intervals in this.

The skeleton measures as follows:

* Organic Remains, vol. iii. † Ossemen's Fossils, tom. iv.

	Ft.	In.
Height to the tip of the horn	10	4
Do. to the point of the spines of the dorsal vertebræ	6	6
Length of the spine.	10	10
Distance between the extreme tips of the horns measured by the skull	11	10
Distance between the tips measured in a strait line across	9	2
Length of each horn	5	9

(Dublin Philosophical Journal.)

8. *Pecten niveus, a new Species.*

It having been suggested, in hasty terms, in the number of the *Annals of Philosophy* for November last, that the *Pecten niveus*, described in vol. xiii. p. 166, of the Philosophical Journal, is perhaps a mere variety of *P. islandicus*, I judge it expedient to institute a comparison between the two species after the manner in which I have compared *P. niveus* with *P. varius*, the only species to which it approaches in its characters. *P. islandicus* has from 70 to 100 or more* ribs; *P. niveus* has invariably 46; † in the *former*, the ribs are very irregularly grouped from 2 to 6 being crowded together, with smaller ones intervening, but without any regularity; in the *latter*, they are beautifully regular; in *P. islandicus* they are marked with very numerous, delicate, erect laminæ, or scales, without any appearance of echinations; in *P. niveus* they are compact and smooth, with scattered echinations toward the margin of the shells; *P. islandicus* is a tolerably thick shell, of a pale-reddish colour, with concentric circles of a deeper tint; *P. niveus* is a very thin shell, of a pure white colour; *P. islandicus* has a margin singularly irregular in its teeth, recalling the idea of that sort of leaf which is termed *folium crispatum; P. niveus* has its marginal teeth as regular as those of a Cockle. If after this *P. islandicus* and *P. niveus* should be considered identical, then assuredly *P. maximus* and *P. jacobæus* are so also; and scarcely any two species of a genus can be named, that must not, on the same grounds, be mere varieties. I now subjoin the distinctive characters of the three species. *P. islandicus*, testâ suborbiculari rubente, fasciis concentricis saturatioribus, radiis circiter 100 varie aggregatis rotundatis lamellulis densissimis scabriusculis. *P. niveus*, testâ orbiculari, fragili candidâ, radiis 46 subcompressis rotundatis sparsim breviter tenuiterque echinatis. *P. varius*, testâ orbiculato-oblongâ, colore variâ, radiis 32, obsoletè squamosis, subcompressis, rotundato-planatis, sparsim crassè echinatis. W. M'G. —(Edin. Phil. Jour.)

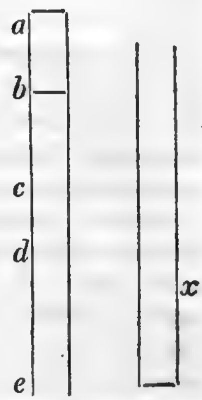
* In a specimen in the Museum of the University of Edinburgh, the number is 104; in a very perfect specimen belonging to W. Nicol, Esq. Edinburgh, the number is 106.

† That is to say in 32 specimens.

MISCELLANEOUS.

9. *Description of a new Instrument to ascertain the Specific Gravity of Powders.*

Prof. Leslie lately exhibited a very interesting experiment in his Class Room, with a new instrument to ascertain the specific gravity of powders, and all kinds of solid substances which will not bear immersion in water.



The instrument consists of a glass tube *a e*, about three feet long, and open at both ends. The wide part *a b* is about four-tenths of an inch in diameter, the part *b e* about two-tenths. The two parts communicate at *b* by an extremely fine slit, which suffers air to pass, but retains sand or powder. The mouth at *a* is ground smooth, and can be shut so as to be air tight by a small glass plate (shown a little above it in the figure). The substance whose specific gravity we wish to find—suppose it to be sand—is put into the wide part of the tube, or the cylindrical cavity *a b*, which may either be filled to the top or not. The tube being then held in a vertical position, the narrow part is immersed in an open tube or vessel *x*, filled with mercury, till the mercury rises both inside and outside to the gorge at *b*. The lid is then fitted on air-tight at *a*. In this state it is evident that there is no air in the tube except what is mixed with the sand in the cavity *a b*. Now suppose the barometer at the time to stand at 30 inches, and that the tube is lifted perpendicularly upwards till the mercury stands in the inside of *b e* at a point *c*, 15 inches* above its surface in the open vessel *x*. It is evident then that the air in the inside of the tube is subjected to a pressure of exactly *half an atmosphere*, and of course it dilates and fills precisely twice the space it originally occupied. It follows too, that since the air is dilated to twice its bulk, the cavity *a b* contains just half of what it did at first, and the cavity *b c* now containing the other half, the quantity of air in each of these parts of the tube is equal. In other words, the quantity of air in *b c* is exactly equal to what is mixed with the sand in *a b*, and occupies precisely the same space which the whole occupied before its dilatation. Let us now suppose the sand to be taken out, and the same experiment repeated, but with this difference, that the cavity *a b* is filled

* When the barometric column is 29 inches, the height is $14\frac{1}{2}$, and so on. This is regulated by a scale.

with *air only*. It is obvious that as the quantity of air is greater than it was when part of the cavity was filled with sand, it will, when dilated to double the bulk under a pressure of 15 inches, occupy a larger space than in the other experiment, and the mercury will only rise, let us suppose to *d*. But let it be remembered that the *attenuated air* in the narrow tube always occupies exactly the space which the *whole* occupied under the ordinary atmospheric pressure. Now this space is in the one case the cavity *b c*, and in the other *b d*. Hence it clearly follows, that the cavity *c d*, which is the difference between these, *is equal to the bulk of the solid matter in the sand*. Now by marking the number of grains of water held by the narrow tube *b e* on a graduated scale attached to it, we can find at once what is the weight of a quantity of water equal in bulk to the solid matter in the sand, and by comparing this with the weight of the sand in air, we have its true specific gravity.

Such is the Professor's process, which appears to us remarkably ingenious, as well as beautifully simple, and we shall see from some of the results which it has already afforded, that it must furnish important aid to the natural philosopher in his researches. On one point a few words of explanation may be required. The Professor is aware that some solid bodies, such as charcoal, hold much condensed air in their pores, and since the probability is, that when reduced to powder they still retain this property in some degree, he obviates the chances of error arising from this source, by comparing the dilatation which takes place under different degrees of pressure—under 10 inches, for instance, and 20, or under $7\frac{1}{2}$ and 15.

Charcoal is known to be precisely the same in its chemical nature with the diamond; but its porosity renders it so light, that the specific gravity assigned to it in books is generally under 0·5, that is, less than half the weight of water, or one-seventh part of the weight of diamond. Prof. Leslie has, however, taken its specific gravity in the state of powder by the instrument we have described, and he finds that its weight actually exceeds that of diamond—that it is one-half greater than that of whinstone—and of course that the substance is more than seven times heavier than it has been hitherto supposed.

The specific gravity of mahogany is generally put down as 1·06. Prof. Leslie finds that of mahogany saw-dust to be 1·68, or two-thirds more. He found that of wheat flour to be 1·56, of pounded sugar 1·83, of common salt 2·15. The last of these agrees very accurately with the common estimate. Writing paper rolled hard up with the hand was found to have a specific gravity of 1·78, and to occupy less than one-third of the space it apparently filled. One of the most remarkable results found, is that relating to volcanic ashes, a substance which seems very

light on a superficial examination, but which was found to have a specific gravity of 4·4. It is, therefore, as heavy as some of our ores of copper and iron. We ought to mention that these numbers were given by the Professor rather as approximations than as strictly accurate results, the instrument first constructed not being quite so perfect as he expects by and bye to render it, and his experiments not having been very numerous.—(Scotsman.)

10. *Preservation of Anatomical Preparations.*

Braconnot has applied the persulphate of iron, in consequence of its astringent and antiseptic properties, to the preservation of anatomical preparations. It combines easily with all the humours and soft tissues of animals, and preserves them from putrefaction and insects. A brain, portions of liver, spleen, and lungs, impregnated with this salt, have a long time resisted destruction. Dr. Macartney, of Dublin, covers his preparation jars with a thin plate of Indian rubber varnished.* This is superior to lead or bladder, and retains the vapour of alcohol perfectly.—(Dublin Philosophical Journal.)

11. *Local Attractions.*

The Connaissance des Tems, 1827, contains an account of Geodetical operations in Italy by the French geographical engineers, remarkable for the discordance it exhibits between results deduced from these operations and from astronomical observations. Of the exactness of the survey no doubt can be entertained from the recital given, and the astronomical results are founded on the observations of several most able astronomers. The discordances, which in one case amounts to nearly 27″ and in another to 17″, are attributed to local deviations of the plumb line, caused by irregular attraction. The matter near the surface at Milan appears to attract the plumb line considerably to the north of the vertical, and that near Remini considerably to the south.—(Dublin Philosophical Journal.)

12. *On the cutting of Steel by soft Iron.*
By Thomas Kendall, Jun.

As the subject of cutting steel by soft iron has excited considerable attention, and seems not yet to be exhausted, I take the liberty to communicate such facts connected with the subject as have come under my own observation, together with some remarks, which are at your disposal. In the cutting of *revolving iron* by tempered steel, experience proves that there is a certain velocity beyond which it cannot be well and freely done. Much depends on the purity and state of the iron; much on the form,

* See *Annals* for January, p. 74.

temper, and sharpness of the cutting instrument; much whether the work is performed dry or kept constantly wet with water or oil; and also much on the *disposition of the particles of iron to chip*. There is a great difference in different samples of iron in that respect, but much more different in copper and its alloys, some of which, although sufficiently soft, can scarcely be wrought by turning, filing, drilling, or grinding. Whenever the steel or cutting tool, from any cause, ceases to act on the iron, and the heat is perhaps at the maximum, the iron, if revolving, will act on the steel; the greater the velocity the more freely it acts, and the progress is marked by different appearances corresponding with the different velocities. In the case of cutting a saw plate with soft iron, if moving with a velocity barely sufficient to act on the steel, this becomes heated beyond the cutting tool to a blue colour; if moving with greater velocity, no change of colour is seen, except on the burr raised by the tool; if with greater still, no change of colour is perceived, although the movement is attended by the *combustion* of most of the particles disengaged. These become ignited because, being connected with, and forming a part of, the plate, they are by the motion disengaged with a velocity that does not admit of the transmission of the heat to the other parts of the steel. Perhaps the ignition is commenced, and carried to that degree denominated black heat, before the particles are separated, and is completed by the friction attending the separation. It is a fact, perhaps not greatly known to those who have written on the subject, that *at the heat called black heat* (but which is in fact nearly or quite a red heat in the dark), *steel is broken or separated by fracture,* with much less force than when heated less or more*, the requisite temperature varying probably in proportion to the carbon contained in the steel.

The result of the copper wheel mentioned by MM. Darier and Colladon having no action on the steel, goes far to prove that the effect depends at least as much on heat softening the steel, to a certain degree, as on percussion, copper having but little disposition to generate heat under any circumstances, a fact duly appreciated by the manufacturers of gunpowder.

The reason why "the heat should be nearly all concentrated to the steel, and scarcely perceptible in the iron," I think to be this; the percussion against the steel is *continual*, but against any one part of the iron cutter, perhaps not more than from $\frac{1}{200}$ to $\frac{1}{600}$ parts of the time; consequently the heat received by each would be in an inverse proportion of the thickness of the steel to the

* The disposition to be easily separated by *fracture* at a *particular heat* exists in carbonized or cast iron, in the alloys of copper and of tin, is very perceptible in flint glass, and perhaps in all factitious metallic compounds; some requiring a moderate, and others a more intense heat.

circumference of the iron, after making the proper allowance for what may be thrown off from the circular cutting iron in its passage through the air, which must be considerable.

P. S. As evidence of the absence of heat, it is stated in the memoir of MM. Darier and Colladon, that the small particles of steel adhering to the edge of the cutter, "seen through a lens, did not appear as if untempered, and when tried with a file were found as hard as the best tempered steel."

I have never observed the appearance of the particles, or examined their temper, but have examined the burr raised in cutting a plate of steel, which, before the operation, was sufficiently soft to file with ease, but in the operation became hardened on the outer edge much harder than before, which was evidently caused by the great heat, and by being suddenly cooled by the current of air caused by the motion of the cutter; the same would be the case with particles disengaged by heat, or when hot, and adhering to the edge of the cutter; the process of hardening in air is applied by artists to the hardening of very small drills.—(Silliman's Journal.)

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

In a few days will be published, a new Supplement to the Pharmacopœias of London, Edinburgh, Dublin, and Paris; forming a complete Dispensatory and Conspectus, including Patent Medicines, Paints, Varnishes, &c. 8vo. By James Rennie, AM. Lecturer on Chemistry, &c.

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The Edinburgh Encyclopædia. Vol. 17, Part 2. 4to. 1*l.* 1*s.*

Article XV.

NEW PATENTS.

J. Fraser, Houndsditch, engineer, for an improved method of constructing capstans and windlasses.—Feb. 25.

B. Newmarch, Cheltenham, for certain inventions to preserve vessels and other bodies from the dangerous effects of external or internal violence on land or water, and other improvements connected with the same.—Feb. 25.

B. Newmarch, Cheltenham, for a preparation to be used either in solution or otherwise, for preventing decay in timber or other substances, arising from dry rot or other causes.—Feb. 25.

J. Fraser, Houndsditch, engineer, for a new and improved method of distilling and rectifying spirits and strong waters.—March 4.

R. Midgley, Horsforth, near Leeds, for a method, machine, or apparatus, for conveying persons and goods over or across rivers or other waters, and over valleys or other places.—March 4.

G. Anderton, Chickheaton, Yorkshire, worsted spinner, for improvements in the combing or dressing of wool and waste silk.—March 4.

J. Neville, New Walk, Shad Thames, engineer, for a new and improved boiler or apparatus for generating steam with less expenditure of fuel.—March 14.

N. H. Manicler, Great Guildford-street, Southwark, chemist, for a new preparation of fatty substances, and the application thereof to the purposes of affording light.—March 20.

Article XVI.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
1st Mon.							
Jan. 1	N W	30·10	29·90	47	36	—	—
2	E	30·10	30·02	46	28	—	
3	E	30·08	30·02	46	33	—	
4	E	30·08	30·06	36	34	—	
5	E	30·06	29·94	38	33	—	16
6	E	30 00	29·94	40	37	—	04
7	N E	30·11	30·00	38	33	—	
8	S E	30·19	30 11	33	25	—	—
9	E	30·11	30·01	33	20	—	
10	N W	30·01	29·88	32	25	—	
11	N W	29·96	29·87	35	18	—	—
12	N W	30·13	29·96	32	17	—	
13	W	30·26	30·13	33	16	—	
14	W	30·43	30·26	24	10	—	
15	N W	30·67	30·43	29	12	—	
16	N W	30·72	30·67	30	15	—	
17	S W	30·72	30·65	33	22	—	
18	S W	30·65	30·28	46	35	—	
19	N W	30·34	30 25	45	34	—	
20	N W	30·35	30·34	46	39	—	
21	S W	30·34	30·31	44	33	·48	—
22	N W	30·35	30·34	42	32	—	
23	W	30·55	30·35	43	33	—	
24	N	30·54	30·49	39	35	—	
25	S E	30·49	30·46	34	32	—	
26	E	30·46	30·43	36	24	—	
27	E	30·43	30·39	36	28	—	
28	S E	30·39	30·26	39	27	—	
29	N	30·26	29·99	43	28	—	
30	N	29·99	29·92	44	39	—	
31	N	29·99	29·99	45	38	·23	
		30·72	29·90	47	10	·71	·20

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

First Month.—1. Rainy morning. 2—4. Fine. 5. Snow and sleet. 6, 7. Cloudy. 8. Fine: very bleak wind, with occasional showers of hail and snow. 9. Fine. 10. Overcast: bleak. 11. Snow. 12, 13. Fine. 14. Foggy. 15. Very fine much rime on the trees. 16, 17. Ditto. 18. Fine. 19. Cloudy: a gentle thaw. 20. Cloudy. 21. Foggy: drizzly. 22—25. Foggy. 26—30. Fine. 31. Overcast.

RESULTS.

Winds: N, 4; NE, 1; E, 8; SE, 3; SW, 3; W, 3; NW, 9.

Barometer: Mean height

For the month.. 30·234 inches.

Thermometer: Mean height

For the month.. 33·193°

Evaporation.. 0·71 in.

Rain.. 0·20

Laboratory, Stratford, Second Month, 27, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1826.

Article I.

On a new Application of the Method of Parameters to the Determination of certain Curves. By Henry Moseley, Esq. BA. of St. John's College, Cambridge.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

If the tangent to any point (X, Y,) of a curve be given in position in terms of the co-ordinates of a corresponding point $(x, y,)$ in another curve, whose equation $f\,x\,y = 0$, is also given ; there exist among the quantities concerned the three following equations by the elimination of (x) and (y), between which the former curve is determined.

$$\mathrm{F\,X\,Y}\,x\,y = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

$$\frac{d\mathrm{F}^*}{d\,x}\,d\,x + \frac{d\mathrm{F}}{d\,y}\,d\,y = 0 \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

$$f\,x\,y = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

For let the tangents to any two points in the required curve intersect in the point (X′, Y′,) ; and let the positions of these tangents be given according to the supposition in terms of (x, y), $(x + \Delta\,x, y + \Delta\,y)$ co-ordinates of corresponding points in the given curve.

There are, therefore given the equations

$$\mathrm{F\,X'\,Y'}\,x\,y = 0$$
$$\mathrm{F\,X'\,Y'}\,(x + \Delta\,x)\,(y + \Delta\,y) = 0$$

From the last, we obtain by expansion,

$$\left.\begin{array}{l} \mathrm{F\,X'\,Y'}\,x\,y + \frac{d\mathrm{F}}{d\,x}\Delta\,x + \mathrm{P}\,(\Delta\,x)^2 \\ \qquad\qquad\qquad + \mathrm{Q}\,(\Delta\,x)\,(\Delta\,y) \\ \qquad + \frac{d\mathrm{F}}{d\,y}\Delta\,y + \mathrm{R}\,(\Delta\,y)^2 \end{array}\right\} = 0$$

* F is here written for $\mathrm{F\,X\,Y}\,x\,y$.

Therefore, by the former,

$$\frac{dF}{dx} + \frac{dF}{dy} \cdot \frac{\Delta y}{\Delta x} + P(\Delta x) + Q' \Delta y = 0$$

Now let the tangents move up to one another until they become consecutive, and ultimately intersect in the point (X, Y,) of the required curve.*

Then since Δx and Δy are evanescent, and X′, Y′, become X and Y, we have,

$$\frac{dF}{dx} + \frac{dF}{dy}\frac{dy}{dx} = 0$$

$$\therefore \frac{dF}{dx} dx + \frac{dF}{dy} dy = 0$$

The above is the Equation (2).

Also by the Equation $F X' Y' x y = 0$, we have

$$F X Y x y = 0$$

which is the Equation (1); and (3) is given by hypothesis.—Q. E. D.

Example 2.—To investigate general formulæ for the determination of the involute to a curve.

Let (X, Y,) $(x, y,)$ be corresponding points in the curve and its involute; and (s) the length of curve measured from the common origin to the point $(x, y,)$; then evidently

$$s^2 = (X + x)^2 + (Y - y)^2 \ldots\ldots\ldots\ldots (1)$$

differentiating with respect to x, y, and s (a function of these variables).

$$\left\{1 + \left(\frac{dy}{dx}\right)^2\right\}^{\frac{1}{2}} = \frac{(X + x) - (Y - y)\frac{dy}{dx}}{\left\{(X + x)^2 + (Y - y)^2\right\}^{\frac{1}{2}}} \ldots\ldots\ldots (2)$$

Eliminating x and y between these equations and the given Equation $y = f x$ of the evolute, we have the required equation (in X and Y) to the involute.

Thus in the cycloid, since

$$s^2 = 8 a x$$

$$8 a x = (X + x)^2 + (Y - y)^2$$

$$4 a = (X + x) + (y - Y) \frac{dy}{dx}$$

Also $\quad \frac{dy}{dx} = \left(\frac{2a - x}{x}\right)^{\frac{1}{2}}$

$$\therefore (y - Y)^2 = \frac{(4a - X - x)^2 x}{2a - x}$$

$$\therefore 8 a x = (X + x)^2 + \frac{(4a - Y - x)^2 x}{(2a - x)}$$

* The curve may evidently be considered as the locus of the ultimate intersections of its consecutive tangents.

$$\left.\begin{array}{l}16\,a^2\,x - 8\,a\,x^2 \\ -\,2\,a\,(X + x)^2 + x\,(X + x)^2\end{array}\right\} = \left\{\begin{array}{l}16\,a^2\,x - 8\,a\,x\,(X + x) \\ +\,(X + x)^2\,x\end{array}\right.$$

$$\therefore (X + x)^2 = 4\,X\,x$$

$$\therefore (X - x)^2 = 0$$

$$\therefore X = x$$

$$8\,a\,X = 4\,X^2 + (y - Y)^2$$

$$\therefore (y - Y) = 2\,(2\,a\,X - X^2)^{\frac{1}{2}}$$

$$\therefore \frac{(2\,a\,X - X^2)^{\frac{1}{2}}}{X} - \frac{d\,Y}{d\,X} = \frac{(2\,a - X)}{(2\,a\,X - X^2)^{\frac{1}{2}}}$$

$$\therefore \frac{d\,Y}{d\,X} = \left(\frac{X}{2\,a - X}\right)^{\frac{1}{2}}$$

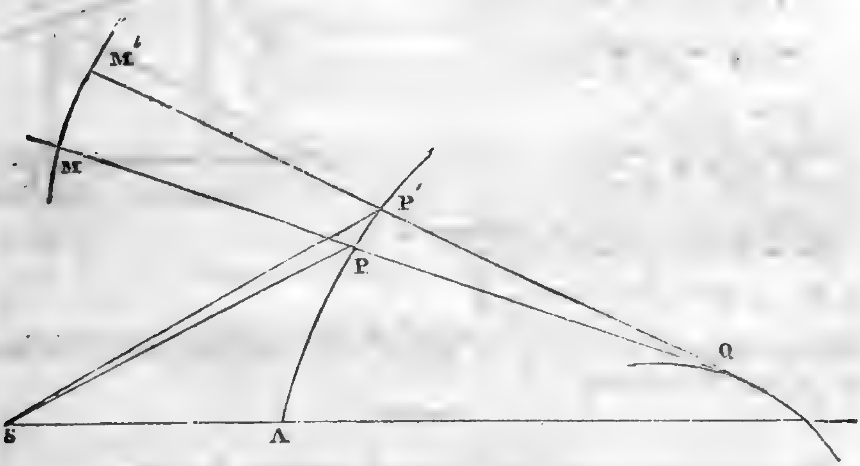
The equation to a cycloid, equal and similar to the former, having its origin at the extremity of the base instead of the vertex,—a known property of the curve.

On the above method of the involute, we may make the following remarks.

1. The involute is in all cases determinable in finite algebraical terms when the evolute is rectifiable.

2. Where the length of the evolute is expressed by a transcendental function, the equation to the involute is always affected by a similar function.

Example 2.—To investigate formulæ determining immediately the involute to a caustic by refraction.



Let S be the radiating point, and A P the refracting surface, and S P, S P′, consecutive rays, refracted so as to intersect in Q. Produce Q P, Q P′, to M, M′, so that P M $= \frac{S\,P}{m}$, P′ M′ $=$ $\frac{S\,P'}{m}$; then since by the nature of caustics, we have ultimately

$$S P + m P Q = S P' + m P' Q$$

$$\therefore \frac{SP}{m} + P Q = \frac{SP'}{m} + P' Q$$

$$\text{Or } P M + P Q = P' M' + P' Q$$

$$\therefore Q M = Q M'$$

and the same being true for all positions of M similarly determined, the locus of M is the involute to the locus of Q, i. e. to the caustic.

Now to determine the locus of M, we have

$$(X - x)^2 + (Y - y)^2 = \frac{x^2 + y^2}{m^2} \quad \ldots\ldots\ldots (1)$$

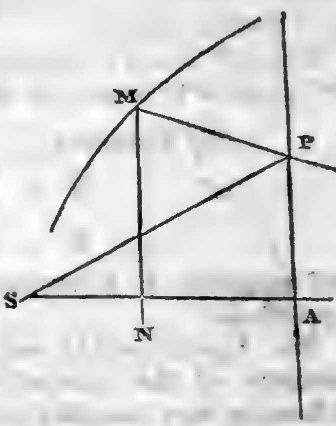
and differentiating with respect to x and y. Since the position (of the normal P M and the point M, and $\therefore$) of the tangent at M is a function of the co-ordinates of P, we get

$$\frac{1}{m^2}\left(x + y\frac{dy}{dx}\right) + (X - x) + (Y - y)\frac{dy}{dx} = 0$$

$$\therefore X + Y\frac{dy}{dx} = \left(x + y\frac{dy}{dx}\right)\frac{m^2-1}{m^2} \quad \ldots\ldots\ldots (2)$$

Eliminating x and y between the Equations (1) and (2), and the given equation to the refracting surface, we have the required equations in X and Y.

Thus when the refraction is made at a plane surface A P; taking A for the origin, if A S = a, we have



$$(Y - y)^2 + X^2 = \frac{a^2 + y^2}{m^2}$$

$$-(Y - y) = \frac{y}{m^2}$$

$$y = \frac{m^2}{m^2-1}Y$$

$$Y - y = \frac{-Y}{m^2-1}$$

$$\therefore \frac{Y^2}{(m^2-1)^2} + X^2 = \frac{a^2}{m^2} + \frac{m^2}{(m^2-1)^2}.Y^2$$

$$\therefore Y^2 = (m^2-1)\left\{X^2 - \frac{a^2}{m^2}\right\}$$

the equation to an ellipse or hyperbola, according as (m) is less or greater than unity.

When m equals unity, the general equations become

$$(X - x)^2 + (Y - y)^2 = x^2 + y^2$$

$$X + Y\frac{dx}{dy} = 0$$

$$\text{Or} \quad X^2 + Y^2 - 2(Xx + Yy) = 0. \ldots\ldots (1)$$

$$X\,dx + Y\,dy = 0. \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

The above are the equations to the involute of a caustic by reflexion.

Example 3.—If a plane curve roll on another similar and equal curve to determine the path of a given point in the rolling curve.

Let there be taken for origin a point in the fixed similarly situated to the given point in the rolling curve. Whence we have

$$(X - x)^2 + (Y - y)^2 = x^2 + y^2$$

Or, $$X^2 + Y^2 - 2(X x + Y y) = 0 \ldots\ldots (1)$$

And since the principle we have demonstrated is manifestly applicable to this case, we get differentiating with respect to x and y.

$$X\,d\,x + Y\,d\,y = 0 \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

Thus if the curves in question be rectangular hyperbolas and the generating point, the centre of the rolling hyperbola

$$x^2 - y^2 = a^2$$

$$\therefore d\,y = \frac{x}{y}\,d\,x$$

and by (2) $$X y + Y x = 0$$

$$\therefore X\,(x^2 - a^2)^{\frac{1}{2}} + Y\,x = 0.$$

Hence we have

$$x = \frac{X a}{(X^2 - Y^2)^{\frac{1}{2}}}$$

$$y = \frac{Y a}{(Y^2 - Y^2)^{\frac{1}{2}}}$$

$\therefore$ by Equation (1), observing that y is greater than Y,

$$X^2 + Y^2 = \frac{2\,(X^2 a - Y^2 a)}{(X^2 - Y^2)^{\frac{1}{2}}}$$

$$\therefore X^2 + Y^2 = 2\,a\,(X^2 - Y^2)^{\frac{1}{2}}$$

the equation to a leminiscata having double the axis of the hyperbola.

The Equations (1) and (2) are precisely those found for the involute of the caustic by reflexion, we have, therefore, the following very singular property.

If a curve similar and equal to the reflecting curve be made to roll in its plane and upon its periphery (similar points having first been in contact), then a point similarly situated to the radiating point will trace out the involute to its caustic. The above property may be demonstrated very simply on principles purely geometrical. The following are examples of its application.

If a parabola be made to roll on another equal parabola, the vertex of the former curve will generate the cissoid of Diocles, the caustic formed by rays diverging from the vertex of the latter curve, and reflected at its periphery, is, therefore, the involute to the cissoid, and similarly the caustic formed by rays diverging from the centre of an hyperbola is the involute to a leminiscata of double the axis.

Article II.

On a Volatile Oil, possessing peculiar Properties, obtained from Soap Ley. By M. Scanlan, Esq.*

In making iodine from "Soaper's Salt Ley," as recommended by Dr. Ure, I obtained a substance not mentioned by him, and which could not have escaped his notice, did the "brown iodic liquor" employed by that chemist afford it.

The soap makers here sell their *salt ley* to persons who evaporate it, separate the muriate of potash, by crystallization, for the alum makers, and when an abundant crop of crystals is no longer afforded, the mother liquor, which contains hydriodate of soda, is run down to dryness, mixed with coal ashes, and fused in a reverberatory furnace. This forms the black ash which is again employed in the manufacture of soap. It is this mother liquor on which I operated; it is loaded with animal matter which is not entirely separated from it by considerable excess of acid.

In the process for obtaining iodine, from this liquor, there distils over a dense oily fluid, of a deep-black colour, immiscible with, and heavier than the acid liquor that rises at the same time. This is the substance in question, saturated with iodine, a large quantity of which it dissolves as it distils. When freed from loosely adhering iodine and washed with water, it has specific gravity 1·39; and rolls about under water with the mobility of mercury. A drop of it in this state, brought to a large surface of water, spreads rapidly over it, and instantly evaporates. Exposed to air on a plane of glass, it evaporates more slowly, the iodine disappearing first. Submitted to distillation, iodine rises first, but is again dissolved by the oily liquor, as it distils. It is soluble in alcohol at ·850, in every proportion, from which it is separated by water, apparently unchanged.

Potash water separates the iodine; forming iodate and hydriodate of potash, the solution of the latter holds dissolved a portion of the oily substance.

Frequent agitation, for a few hours, in contact with iron filings and water, also separates the iodine; and the resulting hydriodate takes up some of the oily matter.

Digested upon iron filings without water, the iodine disappears, but more slowly; and crystals are deposited, which I suppose to be iodide of iron. In neither case is there any perceptible elevation of temperature.

Freed from iodine, by potash water, the colour of this oily matter is yellow; when tasted, the first impression is cinnamon-sweet, afterwards penetrating and stimulant. In this state it is

* Dublin Philosophical Journal.

also soluble in alcohol, and is again separable by water; it readily and entirely evaporates, leaving no stain on paper. It is little inflammable, depositing abundance of carbon, as it burns: the colour of its flame is pale green.* It is soluble in caustic ammonia; it dissolves camphor.

Nitric acid did not inflame it, but produced increase of temperature; and in one instance changed the colour to a beautiful pink. Oxalic acid is formed by the action of nitric acid upon it.

All the iodine I have prepared from soap ley is contaminated with this substance, even when well washed with water, dried on blotting paper, and exposed for some time to the air.

In forming hydriodate of iron with iodine of my own preparation, in order to make hydriodate of potash, I had always observed a smell closely resembling water recently distilled from horse-radish, and an oily appearance on the neck of the flask, which was not the case with iodine bought of Messrs. Herrings and Burbige, London.

To ascertain the cause of this difference, I submitted eight troy ounces of iodine, prepared by myself, with iron filings and water, to distillation in a retort, and obtained about thirty or forty drops of a heavy oil, differing from that first obtained in its colour, which was somewhat deeper, and in smell and taste, both of which were like horse-radish, though not quite so penetrating. A similar quantity of London iodine did not even flavour the water distilled in the experiment.

From the foregoing it appears, that there must be some great difference between our ley and that of England and Scotland, which probably may be accounted for by the fact, that the soap-boilers here use large quantities of fat, partially decomposed in the roasting of meats, in fabricating their soaps; which to those who prepare muriate of potash from soap ley, is a source of annoyance, hindering the free crystallization of the salt, after it has arrived at a certain density.

I can in no way account for the formation of this oily substance, unless it be, that the partially decomposed animal matter above-mentioned enters into some peculiar sort of combination with the hydriodate in the kelp ley, and is subsequently further decomposed by the sulphuric acid, used in eliminating the iodine. If to the concentrated soap ley boiling, a considerable excess of sulphuric acid be added, there separates a black substance, of the consistency of pharmaceutical extract, which, when heated by itself, melts and puffs up, and when cold is brittle like pitch, not unlike the substance which results from the decomposition of alcohol by sulphuric acid, in making sulphuric ether, and perhaps attributable to changes not very dissimilar.

Very complicated effects seem to be produced by the action

* Not unlike chloric ether.

of the sulphuric acid and manganese on the soap ley; for, beside the iodine and oily substance already mentioned, there are formed some white transparent acicular crystals, at the commencement of the process, before any iodine has risen; but they are soon dissolved by water which rises subsequently. And towards the end of the process, some opaque, yellow, arborescent crystals form; the former I could never obtain in quantity to examine; the latter I found to be a compound of iodine and carbon, which I have since formed by the action of iodine and potash on alcohol.*

Article III.

On a new Variety of Wolfram, or Tungstate of Iron.
By M. Vauquelin.†

MM. Delhuyar were the first who found in wolfram the acid which Scheele had discovered in the *tungsten* or *calcareous tungstate* of the Swedes. They discovered moreover that this mineral was mixed with oxide of manganese; but they did not attempt to ascertain the state or proportion in which it existed in it.

Some years afterwards M. Hecht and I performed a fresh analysis of wolfram at the School of Mines, the results were,

Tungstic acid	67
Oxide of iron	18
Oxide of manganese	6
	91

Lastly, M. Berzelius, in his Determinations of the Elements of inorganic Matter, fixed the composition of this mineral as follows:

Tungstic acid	74·666
Oxide of iron	17·594
Oxide of manganese	5·670
	97·930

M. Berzelius, supposing that the iron and manganese existed in the wolfram in the state of protoxides, calculated the relation between the oxygen of these bases and that of the acid.

M. Alluau, sen. who has always, as is well known, much occupied himself with the mineralogy of his country, Haute-Vienne, has lately sent me for analysis, among other minerals, a speci-

* See *Annals of Philosophy*, New Series, No. 55, p. 14.
† Annales de Chimie et de Physique.

men of tungstate of iron, in which he suspected the presence of yttria and columbium. I could not find either of these bodies, but I found the proportions of its elements different from those previously determined to exist in wolfram by the chemists above cited.

In the first analysis, we obtained the following results:

	Peroxide of iron	16·0
	Peroxide of manganese	14·8
consequently	Tungstic acid	69·2
		100·0

On examining the iron, we found that it still contained some manganese, and the latter contained a little carbonate of lime.

In a second analysis, we had

	Peroxide of iron	15·6
	Peroxide of manganese	16·0
consequently	Tungstic acid	68·4
		100·0

Lastly, a third analysis gave,

	Peroxide of iron	13·8
	Peroxide of manganese	13·0
consequently	Tungstic acid	73·2
		100·0

There is, therefore, in this case a larger proportion of manganese than in common wolfram; but the question is, whether it is actually a new variety, or if the difference which occurs is not dependent upon the less perfect methods then employed. It is, therefore, requisite to state how I operated.

1. I fuse the powdered mineral in a platina crucible, with one and a half its weight of caustic potash, adding a little water to facilitate their mixture.

2. When the matter is in a state of very perfect fusion, which occurs readily, I put the whole into water, and keep it boiling for some time to accelerate the precipitation of the oxides of iron and manganese.

3. I draw off the clear liquor by a syphon, and wash the residuum with boiling water till it ceases to be alkaline.

4. Then I dry the oxides at a red heat, and weigh them.

5. I dissolve them in muriatic acid, dilute the solution with water, and add a little carbonate of potash, until the iron is perfectly separated, and the solution is colourless.

6. I filter it through dried and weighed paper, and when it is well washed, I heat it in a platina crucible; but as a small quantity always remains upon the filter, I dry it at a temperature of

212° Fahr. and weigh it again, or I burn it, and also the same quantity of filtering paper, and keep the ashes for a counterpoise of those of the filter.

As to the manganese which remains in the solution, I precipitate it by subcarbonate of potash: it appears in the state of white flocks, which soon diminish in volume and become granular. The vessel must be shaken for some time in order to prevent the substance from adhering to it. I entreat chemists to excuse me for entering into these minute details, but they may not, perhaps, be useless to some persons.

The quantity of the bases here obtained being too great for the saturation of the tungstic acid, the following experiment was made.

Five grammes of wolfram treated by potash gave 1 gramme 59 centigrammes of iron and manganese; the analysis of these 1·59 yielded 69 cent. of iron, 65 of manganese, and 19 of silica; these quantities divided by 5, in order to convert them into hundredths, give 13·8 of iron, 13 manganese, and 3·8 silica; but on examining the manganese, there were found 8 cent. of carbonate of lime, which also divided by 5, give 16 milligrammes to be deducted; the oxide of manganese is thus reduced to 11·4 grammes.

As four analyses were performed which gave the same result within about one-hundredth, and as the above experiment was performed with the greatest precaution, there is ground for hoping that it is as correct as can be expected by chemical means.

In adding together the different quantities of matter which have been mentioned, we get 31·48; but of this quantity there are 4·68 of silica and lime, which unquestionably form no essential part of the mineral: this reduces the iron and manganese to 26·8; thus subtracting these 4·68 of silica and lime, it appears that only 95·32 cent. of wolfram were operated upon, which consequently raises the quantity of iron to 14·462, manganese to 11·949, and tungstic acid to 73·599 cent. On this occasion the analysis of common wolfram was repeated, and the results were the following:

Peroxide of iron	19·5
Peroxide of manganese	5·4
Silica	4·5
Alumina	1·6
	31·0

But subtracting from this amount 6·1 of silica and alumina, which ought to be considered as foreign admixture, the iron will become 20·745, the manganese 5·744, and the tungstic acid consequently 73·511. It is then evident that this kind of

tungstate of iron is different from that previously mentioned, for the quantity of manganese does not amount to one-half; but it is remarkable that the quantity of acid is nearly the same.

Two analyses of common wolfram were performed by directly acting upon it with muriatic acid, and we obtained in one experiment 70, and in the other 72 of tungstic acid. The operation is very long and tedious, because the muriatic acid decomposes the last portions of wolfram with difficulty, and also because the tungstic acid adheres to the sides of the vessel, if it be not very frequently stirred.

When the acid ceases to become coloured, and the residuum is of a pure yellow colour, it must be washed with water, and afterwards repeatedly treated with ammonia; if there remain anything insoluble in the ammonia, as most frequently happens, it ought to be white; this residuum is to be dried and weighed, and its weight deducted from that of the wolfram employed. This method is inexact, unless we ascertain that the muriatic acid has not dissolved either alumina or lime; this is undoubtedly the reason why less tungstic acid was obtained by this method than by potash.

When wolfram in fine powder is long digested in muriatic acid, it is entirely decomposed; but in this case, the quantity of acid must be large, and it must be very strong. When the ferruginous solution is poured and mixed with water, it becomes milky, and deposits a flocky yellow matter, which is tungstic acid: when this has been well washed, it would seem that its purity might be depended upon; it contains, however, a little iron, for if it be dissolved in ammonia, it assumes a blue tint, and leaves after solution a yellow substance, which is oxide of iron.

It thus appears that muriatic acid, whether cold or hot, never completely decomposes tungstate of iron: it forms a supertungstate, a portion of which dissolves in the muriatic acid when it is sufficiently strong, and its quantity large; the reason why tungstic acid becomes blue when it is dissolved in an alkali is undoubtedly because a small quantity of tungstate of iron is re-formed; and this being decomposed by the excess of alkali, oxide of iron is left.

Wishing to know the absolute quantity of oxygen contained in wolfram, I reduced 100 parts of common tungstate by a strong heat; it lost 40 parts. In another experiment made nearly at the same temperature, but longer continued, the loss was 46 per cent. The iron which this mineral contains being 20·745 per cent. would suffer a loss of 4·50; and the oxide of manganese being 5·4, it would lose 52 centigrammes, which amount together to 5·09, the remainder of the loss, amounting to 32·91, must have been derived from the tungstic acid.

As the oxides of manganese and iron amount together to

26·945, the tungstic acid ought to make up 100, that is to say, 73·885; this comes very near the result obtained by Berzelius, who estimates the proportion of tungstic acid in wolfram at 74·666. These 73·885 would contain 34·96 of oxygen, 100 of this acid consequently contain 47·20.

In making this calculation, I suppose that 100 of protoxide of iron contain 22·57 of oxygen, and that it is in the state of protoxide that it exists in the tungstate of iron; I also allow that 100 of protoxide of manganese contain 28·107 of oxygen, according to Arfwedson; these results do not, however, at all agree with the analyses of MM. Delhuyar, Bucholz, and Berzelius, all of whom allow only a fifth of oxygen in the tungstic acid. I nevertheless believe that I did not lose any thing in my experiments, the metallic mass obtained being very even, and perfectly entire. I shall, moreover, mention how I operated: the wolfram was put into a small charcoal crucible, placed in another of charcoal, and with a stopper of the same material, and these were introduced into a Hessian crucible, and heated in a forge for an hour.

Not knowing to what to attribute this difference, I imagined that part of the tungstic acid had been volatilized. To satisfy myself respecting it, I heated some tungstic acid in a moderate fire, but sufficient to reduce it, and I had actually a loss of only 20 per cent.; but having another time submitted it to a very strong and long continued heat, it lost 30 per cent. A portion of the metals was, therefore, volatilized.

Although I have supposed in my calculation that the iron in the wolfram is in the state of protoxide, yet some appearances which this metal exhibited in the course of my analyses left some doubts on this subject; I thought it proper, therefore, to subject them to the test of experiment, and the results obtained were as follows:

Into a solution of one gramme of crystallized sulphate of iron, I poured a solution of gold, as neutral as possible; there were obtained 22 centigrammes of gold; I also added solution of gold in excess to the solution of iron obtained by muriatic acid from five grammes of wolfram. The quantity of metallic gold was 37 cent.; but in a gramme of crystallized sulphate of iron, there is about 26 cent. of protoxide, and in five grammes of wolfram there is at least 72 cent. of the same oxide; this quantity of iron, if it were in the state of protoxide, according to the above proportions, ought to precipitate 76·154 grammes of metallic gold, and it gave only 37: there is, therefore, only about half the iron contained in the wolfram which is in the state of protoxide. It will be, perhaps, objected, that during the decomposition of the wolfram, which can be effected only with the assistance of ebullition, a part of the iron is peroxidized; but this operation being effected by muriatic acid, which is concentrated, and in

great excess, there is not much probability of this effect being produced. Besides when wolfram in impalpable powder is digested in cold concentrated acid in a vessel from which the air is excluded, it imparts to the acid a deep-yellow colour; and this, it appears to me, would not happen if the iron were entirely at a *minimum*. If what I have now stated be correct, it results that the relation of 1 to 3 between the oxygen of the bases and that of the tungstic acid is not exact, at least in this kind of combination.

Article IV.

The Influence of Chemistry on the Affairs of Life considered: in a Series of Essays. By Edward B. Stephens, Chemical Assistant to the Royal Dublin Society.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

Essay 1.—*Its Effects on the Human Character.*

Amidst the branches of useful knowledge engrafted on the understanding, chemistry flourishes conspicuously. The mental energies it developes, and the physical benefits it incessantly yields, render it alike interesting to the artist and the philosopher.

Those who are unacquainted with this engaging science have no conception of the enlarged field of thought which it displays, or the power it bestows, to unveil the mysteries, and wield the energies of nature. The value of life is doubled to the chemist, for a new world is opened to him. He is gifted with the power of effecting at will a secondary creation; whilst every step he ascends up the hill of science reveals new prospects, impresses on him more clearly the idea of his natural ignorance, and the extent of the regions of knowledge to be yet explored, and thus insures his humility. Habitually led to reflect on the outward source of all his notions and knowledge, he cannot indulge the unphilosophic thought, that any discovery or invention of his is a proper subject of pride.

The unlimited abundance of the chemist's materials tends greatly to facilitate his accurate and practical knowledge of the science. Every animal,—each leaf, fruit, and seed,—nay, every stone which the earth presents, may be made the subject of an instructive lesson. The pupil may see, feel, taste, or smell, the objects of his investigation, and form such an effectual association of ideas on any subject he is interested in, that memory has no labour to perform. His information is accumulated without the painful effort which accompanies the getting a task, and which in classical and legal acquirements is emphatically termed

study. His business is not with abstract thoughts or words alone; the whole material world is his cabinet; his hands are actively employed, while his observation is pleasingly excited; and the various remarkable facts and natural wonders presented in the course of his experiments, become stored in his memory with the same facility that he notes interesting occurrences of ordinary life.

Although the information which chemistry leads to is inexhaustible, it fortunately happens that a pupil can acquire a precise notion of its nature and objects at the very first. After a few hours familiar explanation of its principles, and of the generalities relating to combustion, earths, metals, acids, alkalies, &c. he may proceed by himself to investigate any particular part of the science he is interested in (manufacturing, medical, agricultural, mineralogical, &c.); whereas in other branches of knowledge, for instance, language, a teacher is as necessary as a book, and the student is not competent to write an essay, or venture into a conversation, till he is familiar with all or most of the words and idioms composing it. As the chemical pupil advances, he habitually attains what Benjamin Franklin would have called the *morals of chemistry*, and in proportion to his devotion to the science, acquires a character strongly tinctured by the favourable circumstances peculiar to it. Such habits (unlike those generated by abstract studies, which frequently unfit a man for social intercourse, and for the active cares of common life) are all highly favourable to the attainment of eminence in any other science or profession. As education and the formation of character are now subjects of universal and well-merited attention, it may be productive of general advantage to review in detail all the valuable habits which become gradually established in the mind of a youth who enters with spirit on the acquisition of chemical knowledge.

The feeling which is first roused to action is *curiosity*, to know the cause of striking phenomena, and the way to produce each brilliant effect at pleasure. Whilst his mind is thus open to the influence of admiration, many useful collateral truths may be instilled with ease; these being associated with pleasurable ideas generally make a lasting impression. In repeating interesting experiments with his own hands, he soon becomes practically convinced of the necessity of acquiring dexterity, and of the comforts that spring from neatness. Continually exposed to accidents which occur like summary penalties on the instant, he abates his cautiousness or self possession; he finds himself in an excellent practical school for the acquisition of both; and generally attains considerable proficiency, sooner than would seem possible to any one accustomed to create these qualities by precept alone.

His first experimental efforts will probably be rendered abor-

tive by his eagerness to pursue several interesting objects at once; but he will soon learn by experience the valuable truth which constituted the chief rule of the great De Witt's policy, that he can only succeed *by attending to one thing at a time.* This habit attained, he is on the high road to knowledge, but several others are essential to success; patience and systematic perseverance in research, cleanliness and a love of order in apparatus and materials, are useful in every operative pursuit, but indispensible in this. His brief records require at the instant an exertion of mental exactness, comprehensiveness, and perspicuity. He must continue in the hourly practice of these humble virtues, to derive any real benefit from his labours, and to avoid being blocked up at the end of each day's work with a mass of unsightly and unserviceable matter.

The delightful mental excitement which sustains and repays him throughout a course of experimental investigation, is nearly allied to mingled feelings of curiosity and admiration, which, in the perusal of a novel, charms his attention to its successive incidents. He is led on through adventures and difficulties—through doubts, mysteries, and partial discoveries, ardently engrossed by his subject, and confident in the consistency of the author, who rewards his perseverance by permitting gleams of light at intervals to dawn upon his anxious mind,—now faint,—now strong, till the long looked-for explanation clears all up at last. But here the similitude ends. The mind of the novel reader sinks into comparative languor when his pursuit is ended, and excitement over; whilst the chemist is roused to higher efforts of discovery by the new information and power which each step in his research has rewarded him with. The former is a species of a passive mental gratification that weakens the taste for profitable or useful employment; whilst the delight enjoyed by the chemist admits of being heightened by cheering reflections on the successful exertion of his active faculties, the certainty that his labours will be useful to mankind, and the hope that they will be productive to himself of character, perhaps of fortune. He finds his mind strengthened by exercise, enlarged by experience, gratified by discovery, confident from past success, and in a state of complete preparation to enter again the field of useful inquiry.

In going through a course of experimental chemistry, the pupil will find himself obliged to abandon so many vulgar errors and popular prejudices, that he becomes cautious how he adopts opinions on physical subjects, without receiving the best proof of their correctness that the nature of the case admits of. He learns to distinguish between what he knows himself, and what he hears of. He finds that besides the propositions which he can believe and disbelieve, there are others respecting which his limited knowledge does not afford him an opportunity of coming

to any conclusion excepting this, that he is totally ignorant of the matter, and that therefore it becomes him to keep his mind open to conviction, and in a state of philosophical impartiality, till he acquires the requisite information. Where the subject admits of experiment, the folly of wordy controversy becomes so apparent that he loses no time by engaging in it: where it does not admit of proof, the folly of positiveness is still more evident. He learns to think and speak of things by weight and measure, and to affirm respecting them no more than he knows; for in this science (unlike those which subsist on opinion) an inaccuracy is an untruth, and if he value his character, he will carefully avoid it by renouncing mere assertion and guess-work.

Chemistry is, in short, the true offspring of the Baconian system of creating knowledge, the *essence of the experimental philosophy;* and a youth placed within its sphere of action (unless he be a grade lower in the creation than his fellow men) cannot long remain unbenefited by the excellent train of thought and action which it certainly induces. Every new truth which enlightens his understanding enables him to judge more correctly of his previous errors, to doubt his own infallibility, to encourage the thought with pleasure (so irksome to many) that he is wiser to day than he was yesterday, and to behold with admiration and profit the works of nature in progress around him, which formerly passed unheeded before his eyes, or conveyed only vague notions to his understanding. If the cultivation of any particular science tend more than another to fix in the mind a rational certainty on religious subjects, and substitute confidence in place of doubt, that science is undoubtedly chemistry. When a man remains incredulous and sceptical on matters of eternal importance, we generally find that this happens because his mind is unfurnished with any certain scientific mode of attaining conviction of the truth, and proving the fallacy of visionary objections. The chemist need never remain in doubt, for he has habitually acquired a readiness of research, and a teachable spirit of humble inquiry, most favourable to the discovery and acknowledgment of truth;—even when its admission lowers his self opinion, destroys his worldly fame, and lays all his cherished theories prostrate in the dust.

The time spent in the acquirement of a general knowledge of the science cannot be deemed a loss in any case; for in this age of chemical invention, its importance is so manifest, that every gentleman is expected to know something of it (and the earlier in life it enlightens his mind the better); and in numerous female academies, it is now adopted as a necessary part of a lady's education. This is a wise arrangement; for (leaving more obvious economical advantages aside) the daily portion of happiness enjoyed by either sex depends so much on finding in each other a similarity of tastes, and opinions, and of knowledge

interesting to both, that every opportunity should be seized on to render available a source of enjoyment so pure and enlightened.

But to return.—The performance of a new analysis is in itself a course of practical education, well calculated to illustrate the favourable circumstances in which a chemical pupil is placed for the attainment of valuable habits. His first care is to arrive at all the precise information which books afford respecting the genus and species to which the substance under examination belongs, as well as the results of those who have preceded him in similar attempts. In this research he is led to compare the style and matter of various authors, and ascertain the rise and progress of truth and error; the former he perceives has been attained only by patient experiment, the latter invariably has sprung up wherever a point was taken for granted. This is an excellent lesson for him, and it is rarely forgotten when he undertakes a similar task.

When thrown on his own resources to perform his analysis, this apparently simple task often gives scope for the exertion of the highest species of invention. He summons up every analogous fact previously committed to memory, reviews the whole store of his knowledge of nature applicable to the present emergency, and selects the re-agents and modes of action apparently most suitable. Failing, perhaps, in these, after various trials, he must again return to the review, and investigate with patience and candour all the probable sources of error in his experiments, as well as in the analogical reasoning which led to them. All this is a work which requires for its execution considerable industry, discrimination, and accuracy, both in observing results, and in drawing conclusions from them. His powers of transition and abstraction are called into life to attend instantly to successive appearances, and consider them carefully ere they pass away. Forethought in planning and perseverance in executing operations of difficulty (perhaps of danger) are alike indispensible to attain satisfactory results. These valuable qualities, which are here but slightly touched on, are all properly attainments, and are as naturally produced by use and the force of circumstances as strength is acquired by the sailor's arm, and the mountaineer's knee. Like exotic plants they may be altogether strangers to the soil, but cultivation can make them take root, and experience proves that under the mental training induced by chemistry, they thrive and flourish.

In drawing up the report of his experiments, the pupil has the advantage of excellent models to form his taste, and excite his literary emulation. These are for the most part characterized by accuracy, simplicity, perspicuity, and modesty. In the analytic essays of Klaproth, for instance, he will find the most valuable discoveries successively communicated with an absence of pre-

tension which leaves him in doubt which most to admire, his profound sagacity, incessant industry, or engaging modesty. With such patterns in view, he will be in no danger of descending to boasting, mystery, or any of the shallow expedients resorted to by those who thus endeavour to conceal their ignorance. He will observe that the best informed men are ever the most ready to publish their disappointments and mistakes, and save to others that time which they had themselves lost.

Perhaps the most valuable result attending the pursuit of chemistry is the strong incentive it creates in the pupil to possess himself of a respectable share of knowledge of the several sciences closely connected with it. Geometry will enable him to enter clearly into the important study of crystallography. When he understands algebra thoroughly, he knows he will be able to pursue the intricacies of the atomic theory. Mineralogy, from whence he derives his chief materials, has peculiar attractions, and strong claims on his notice. Geology is so interwoven with the latter science that his knowledge of either is not complete till he has attained both.

He already possesses the key to medicine, agriculture, and the various arts and manufactures, whose materials and products constantly form the subject of his investigations. In any or all of these, he may acquire pleasing and profitable information, by pursuing the plan already found to be indispensible in chemistry, *attending to one thing at once.*

Thus it appears that this favoured science, liberal, accurate, and extensive, is also the true foundation of many arts and sciences indispensible to our well being; and when we further reflect on the valuable course of mental exercise which it peculiarly induces, we may justly award it a high rank on the list of knowledge essential to a liberal education.

To conclude, the chemist possesses advantages over the members of nearly every other profession, and the students of most other sciences; he need never want pleasing occupation. He claims a subject in every thing he can touch. He need not travel in search of interesting materials to act upon, he cannot travel beyond their reach. He has the power of befriending all men; for none can attain a station which will place them above the want of chemical information. He feels an interest in every conversation, from the most trivial to the most profound; from the composition of the painted cup, or of the social beverage it contains, to the effects of that powerful agent which, directed by the genius of Watt, is rapidly changing the state of the civilized world.

Article V.

On the Classificatian of the Strata which appear on the Yorkshire Coast. By the Rev. A. Sedgwick, FRS. Professor of Geology in the University of Cambridge.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Feb.* 20, 1826.

The following remarks on the classification of the strata which appear on the Yorkshire coast, are principally compiled from *memoranda* which I made during an excursion to that part of our island in the year 1821. It was at first my intention to examine in detail all the natural sections which are exhibited between Flamborough Head and Huntcliff; and afterwards to trace, as far as I could, the more characteristic beds through the Hambleton hills, the Vale of Pickering, and other places in the interior of the district. I had, however, hardly commenced the excursion, when I found that Messrs. Young and Bird, of Whitby, had nearly completed a work devoted to an illustration of the geological structure of the north-eastern parts of Yorkshire. This information induced me to shorten the task which I had proposed to myself, and to confine my observations to the immediate neighbourhood of the coast. I have thought it right to state thus much respecting the materials from which the following remarks are compiled: otherwise the reader might suppose them the result of a more elaborate examination of the district, and on that account be disposed to give them more importance than they in truth deserve.

The work to which I have alluded appeared in the year 1822; and, with many excellent details, gives a history of the structure of the whole Yorkshire coast. No attempt is, however, made in it to connect the phenomena which are so well described, with the corresponding phenomena which are presented in other parts of England. I venture to hope, that the following observations (which I have drawn up for insertion in the *Annals of Philosophy*) will, in some measure, contribute to supply this deficiency, or at least be the means of calling the attention of those who have better opportunities of local information to the same subject.

I have the honour to be, Gentlemen,

Your most faithful servant,

A. Sedgwick.

Sect. 1.—*Difficulties in classifying the Strata of the Yorkshire Coast.*

In examining the structure of the Yorkshire coast, we fortunately meet with no difficulty in determining the true relation which the great mineral masses bear to each other. For the

order of the successive formations, though sometimes disguised by contortions, or interrupted by dislocations, is so plainly seen in many fine natural sections as not to remain in any doubt. If we attempt to connect the formations in question with the contemporaneous deposits in the more southern parts of England, the task, though encumbered with some difficulties, may soon be reduced within very narrow limits. For the deposits in the north-eastern parts of Yorkshire are superior to the *new red sandstone*, which runs in one continuous mass from the banks of the Severn to the mouth of the Tees; and they are inferior to the *chalk* which terminates in the bold escarpment of Flamborough Head. Do they then admit of any natural subdivision which will enable us to compare them with the several formations, between the *new red sandstone* and the *chalk*, which have been described with so many excellent details by Mr. Conybeare? That there is some difficulty in answering this question is obvious from the fact, that in the description of the district in question, our best geological authorities are at variance among themselves.

Mr. Smith identifies the alum-shale of the Yorkshire coast with the Oxford clay; and the coal formation of the moors and the limestone of the vale of Pickering, with the calcareous grit and oolite of the coral-rag formation. Mr. Greenough refers the alum-shale to the lias, the sandstone and coal formation of the moors to the great oolite, and the limestone of the vale of Pickering to the coral-rag. Mr. Conybeare is inclined to identify the formation of the moors with the inferior oolite, and the limestone of the vale of Pickering with the great oolite. A part of these discrepancies has undoubtedly arisen from the anomalous character of the strata in question; for there is obviously great risk of error in attempting to bring into the same class distant deposits which are unconnected, and bear little resemblance to each other. Notwithstanding these difficulties, I am disposed to think, that a careful examination of a very small number of facts, in addition to those with which we are now acquainted, would enable us to determine with certainty the respective places in the series of English formations to which the strata on the Yorkshire coast are to be referred.

The whole country bordering on this part of the coast is naturally divided into several distinct regions, each possessing a distinct geological structure. 1. The low region extending from the southern extremity of Holderness to Bridlington. 2. The chalk downs terminating in Flamborough Head. 3. The low argillaceous region constituting the vale of Pickering. 4. The broken mountainous tract extending from the vale of Pickering to the end of Huntcliff. 5. The low region stretching from the mouth of the Tees towards the central plains of Yorkshire.

In describing the deposits which occupy, and give the character to, these successive regions, I shall endeavour to avoid all details which are not absolutely necessary to my present purpose; as my only object is to ascertain the relations of certain phenomena which have already been elaborately and faithfully described in a work to which I have before referred. On this account, I shall notice the successive formations in the order in which they are described in the "Geological Survey of the Yorkshire Coast," beginning with the most superficial, and ending with the lowest strata of the district.

SECT. 2.—*Strata of Holderness.*

Nearly the whole of this first region is composed of *diluvial detritus.* Some account was given of the deposit in a paper (published in the *Annals of Philosophy* for July, 1825), in which I endeavoured to ascertain its connexion with other similar deposits in the neighbouring parts of England. Any further notice of it in this place is unnecessary.

In so extensive a district, composed exclusively of materials which are superior to the chalk, some traces of *tertiary* beds might be expected to appear in the sections on the coast. But no traces of them are seen in any part of the cliffs south of Bridlington. In some places, however, a very impure variety of coal is occasionally cast up on the beach, which may, perhaps, be derived from some inferior carbonaceous beds similar to those which abound in many parts of the plastic clay formation.*

By the degradation of the coast immediately on the north side of Bridlington Quay, a bed of green-sand, extending through a distance of eight or ten yards, was laid bare at the base of the cliff.† It contained many incoherent bivalve shells, and very much resembled some of the beds which are found in the higher part of the sand-pits of Woolwich. I considered it an undoubted proof of, at least, a partial existence of *tertiary* beds in that part of England. With the *crag* of Suffolk, it ought not, I think, to be confounded.

Mr. Smith, in his Geological Map of Yorkshire, mentions the occurrence of *crag shells* and sand in the interior of Holderness. I have not seen any of the deposits to which he refers, and am, therefore, unable to determine whether they are to be classed with the Suffolk *crag,* or with the shell-beds of the plastic clay formation. Mr. Smith's system of arrangement unfortunately does not enable us to make the separation.

* These varieties of impure coal must not be confounded with specimens of pure pit coal, which are occasionally found on the beach, having been wrecked or scattered on the coast.

† I have been informed that this bed of green sand is now concealed by some works constructed, in the year 1821, for the purpose of preventing the encroachments of the sea.

Sect. 3.—*Chalk of Flamborough Head and the Wolds.**

The chalk undoubtedly forms the foundation on which rest all the beds described in the last section. All its characters are exhibited in a succession of lofty and finely indented escarpments which extend from Bridlington, round Flamborough Head, to Speeton. It is, with few exceptions, more compact than the corresponding beds in the southern parts of England; and the imbedded flint nodules are generally more compressed, often putting on the appearance of large tabular masses, which are striated parallel to the planes of stratification. These flints are of an unusually pale colour, and exactly resemble specimens derived from the chalk in several parts of France. In the chalk marl (which here, as in other parts of England, exists near the bottom of the formation), are many beds of a bright red colour, which alternate with others where the colouring matter is less abundant. This group gives a very singular character to the lower portion of the cliff, especially near Speeton.†

A striking phenomenon connected with this formation, and not, I believe, noticed in any geological work, may be seen in the lofty cliff about half way between Flamborough Head and Speeton. The beds of chalk and flint are at once bent out of their ordinary position, and twisted into a number of curves, the flexures of which are as violent and irregular as those which commonly occur in formations of greywacké. The cause which produced this very singular irregularity must have acted partially; for in the remaining part of the cliff, the beds of chalk and flint have their usual direction and inclination.

Notwithstanding the anomalies which are above stated, the chalk formation of the Yorkshire coast agrees in all its general characters with the same formation in other parts of England, admits of the same subdivisions, and contains the same suite of organic remains. Exclusively, therefore, of the fact of actual continuity, the several parts of the formation just described, must be considered, by every one who is capable of appreciating physical evidence, to be contemporaneous with the several parts of the same formation in the southern districts of our island.

Sect. 4.—*Vale of Pickering.*

The whole of the low region stretching through the vale of

* For a detailed description of this formation, I must refer to the "Geological Survey of the Yorkshire Coast," (p. 46—55). Its superficial extent may be seen in any of our geological maps.

† The red beds in Hunstanton cliff on the coast of Norfolk (which I had the advantage of examining last spring in the company of Dr. Fitton) do not so unequivocally belong to the chalk formation, but rather seem to represent, in a very unusual form, the *Cambridge galt;* for they exist under the chalk formation, and do not alternate with any part of it; and they contain the peculiar fossils of the *galt* in great abundance, and are seen to repose on the green and ferruginous sands (exactly resembling those at Shanklin, in the Isle of Wight), which always have their place below the *galt*.

Pickering, and bounded by the chalk downs on one side, and by the hills of oolitic limestone on the other, is composed of one great argillaceous deposit, which at the north end of Speeton cliff is seen to rise immediately from under the formation described in the preceding section. The junction on the coast is in some measure obscured by a partial dislocation of the strata; but the relative position of the chalk and inferior clay has been unequivocally proved by an actual *boring in search of coal* conducted in the neighbourhood of Staxton, as well as by an examination of several undisturbed sections in the neighbourhood of Knapton and Settrington. (See the "Geological Survey of the Yorkshire Coast," p. 56—60.) From the dip of the oolitic limestone on the north side of the vale, we may infer with perfect confidence that it is inferior to the Pickering clay, though the two formations are not seen in immediate contact on the coast—a conclusion which is confirmed by the arrangement of the several formations, on the south-east bank of the Derwent, below Malton. If then (as I shall endeavour to show in the next section) the oolitic limestone of the Pickering hills be identical with the middle oolite, or coral rag, of Mr. Conybeare's series, it follows that the clay of the vale of Pickering must represent one or more of the known formations between the coral rag and the chalk.

Since the publication of Dr. Fitton's excellent paper in the *Annals of Philosophy* for November, 1824, there is no longer any doubt or difficulty respecting the true arrangement of the beds on the south coast of our island, between the chalk and the Purbeck limestone. All those who had previously written on the subject had been led into occasional errors by sometimes confounding the Shanklin-sand (lower green-sand or ferruginogreen-sand of Mr. Webster) with the beds of green-sand which have their place between the chalk and the Cambridge galt; and at other times by embodying the Shanklin-sand as one formation with the lower Hastings beds. Now that these errors are corrected, the following arrangement of the deposits between the chalk and the coral-rag oolite may be considered as completely established.* 1. Chalk. 2. Green-sand. 3. Cambridge galt.

* Since the publication of the paper above-mentioned, some questions have arisen respecting the appropriate names to be given to the several deposits between the chalk and the Purbeck limestone. Though it is quite foreign to my present purpose to involve my name in any part of the controversy, yet I cannot help expressing my opinion that the term *green-sand* ought to be retained, and applied *exclusively* to the beds between the galt and the chalk. The term was, I believe, used with this limitation by Mr. Smith; and it was used consistently in the same sense by Mr. Webster, in his description of the Isle of Wight. A subsequent, and, it now appears, an erroneous application of the same term to designate other beds below the galt, is no reason for abandoning a name which has been long received and is highly appropriate. Dr. Fitton was unquestionably the first who correctly separated the deposits immediately below the galt; I have, therefore, thought myself bound to follow the name (Shanklin-sand) which he proposes for the highest of them. In a paper on the Isle of Wight (published

4. Shanklin sand (ferrugino-green-sand of Mr. Webster). The three preceding of marine origin, and very nearly related to each other. 5. Weald clay. 6. Hastings sands. The two preceding, with a very peculiar suite of organic remains, chiefly of freshwater origin. 7. Purbeck limestone: in zoological characters approaching the two preceding; but in mineralogical character more nearly associated with No. 8. 8. Portland oolite. 9. Kimmeridge clay. 10. Coral-rag oolite.

With what part of the preceding series shall we then identify the clay of the vale of Pickering? To answer this question correctly, it is obviously necessary to determine the physical characters and the organic remains of the formation. Unfortunately the sections on the coast, disguised by enormous accumulations of *diluvium*, give us much less information than might have been anticipated. I may, however, venture to assert, that the fossils of the argillaceous cliff which succeeds the chalk are not identical with the suite of the Cambridge galt; and that some specimens which I found *in situ*, and others derived from the same locality, which I have seen in private collections, are identical with a part of the fossils of the Kimmeridge clay in the neighbourhood of Weymouth. The general agreement of the Kimmeridge and Pickering clays is further confirmed by the near agreement of their mineralogical characters, and by the beds of bituminous shale which are subordinate to both of them.

Allowing, however, that a portion of the Pickering clay is the true representative of the Kimmeridge clay, it may still be asked, whether the upper part of it may not represent some of the other beds between No. 1 and No. 9 of the general series abovementioned. As far as regards the immediate vicinity of the coast, I am disposed to answer the question in the negative, for the following reasons: 1. There are, I believe, no traces of the Portland and Purbeck beds north of the Humber.* 2. The Weald clay and the Hastings sands seem to constitute a local deposit confined to the south-eastern parts of England; and being principally of freshwater origin, cannot in any way be co-extensive with the marine formations which are above them

in the *Annals of Philosophy* for 1822), I made some remarks on Mr. Greenough's geological map, which I take this opportunity of correcting. At that time I was unacquainted with the coasts of Kent and Sussex, and confounded the Kentish rag (which I only knew by description) with the green-sand rock of the Undercliff in the Isle of Wight. I now think that on any geological map of an ordinary size, it would not be expedient to subdivide the colours further than has been done already; and that the green-sand, the Cambridge galt, and the Shanklin sands, might be conveniently represented by one colour with a designation applying to the whole group. This appears to have been very nearly the system of Mr. Greenough.

* If this assertion be true, it by no means follows that the same formations are wanting on the west side of the wolds of Lincolnshire. A correct description of the beds between the chalk and great oolite in that county is at present a desideratum which, we may hope, will be soon supplied by some of the members of the philosophical societies in the north of England.

and below them. It is, therefore, *à priori*, improbable that they should reappear on the Yorkshire coast. 3. In the section visible in the cliff near Speeton (and also at Knapton, as appears by the "Geological Survey of the Yorkshire Coast," p. 58), the chalk marl rests immediately on the Pickering clay without any trace of the Shanklin-sand, or of the green-sand. It is, therefore, probable, that the intervening *galt* has also disappeared.* I, therefore, conclude, on a review of the best evidence with which I am acquainted, that the whole of the Pickering clay on the coast is to be identified with the Kimmeridge clay; and that all the formations, from No. 2 to No. 8 inclusive, are wanting in the Yorkshire section.

Sect. 5.—*Oolitic Limestone, &c. of the Vale of Pickering.*

The high broken tract of country, extending from the Vale of Pickering through the north-eastern moorlands of Yorkshire to the extremity of Huntcliff, has already been considered as one physical region. It will, however, be convenient to divide the strata which successively present themselves in this region into three distinct groups or formations. 1. A formation of oolitic limestone and calcareous grit. 2. A coal formation. 3. A great argillaceous deposit (alum-shale or lias), immediately inferior to the two preceding, which occupies a considerable portion of the cliff between the Peak of Robin Hood's Bay and the village of Saltburn, and forms the base of the hills which overlook the plain of Cleveland. A short account of these groups will form the subject of the three following sections.

The calcareous grit, constituting the lower portion of the first group, is finely exposed in the promontory of Filey Bridge, and a little further to the west is surmounted by beds of oolitic limestone. The whole group is thence prolonged about thirty miles, in a direction nearly due west, to the escarpment which overhangs the plains of Thirsk. The formation afterwards ranges in a south-easterly direction to Malton, where it crosses to the left bank of the Derwent, and for some way runs nearly parallel to the chalk downs. Farther to the south it disappears for many miles under the chalk; but it afterwards reappears (according to the geological map of Mr. Smith) at Newbald, and is thence prolonged, in a direction parallel to the general bearing of the other strata, to the north bank of the Humber.

* From the geological map of Mr. Greenough, as well as from Mr. Smith's geological map of Yorkshire, we might suppose that some of the sand beds belonging to the group immediately inferior to the chalk, extended without interruption at the base of Yorkshire wolds. How far the delineation is hypothetical, I am not prepared to say; but I saw no trace of the group in question, and it seems to have been unknown to the authors of the "Survey of the Yorkshire Coast." In the geological map of Yorkshire, the clay of the vale of Pickering is called the Oaktree-clay, a term which conveys no definite information; for in Mr. Smith's system, the Weald clay and the Kimmeridge clay are unfortunately confounded under the common name of Oaktree-clay.

By a comparison of the dip and direction of the several parts of the formation just described it appears, that from Filey Bridge to Malton, the beds of oolite form a kind of basin which supports the argillaceous deposit described in the preceding section. For many of the details connected with the natural history of this formation, I must however refer the reader to the "Geological Survey of the Yorkshire Coast," p. 61—69. The *out-lier* of Scarborough Castle Hill, another *out-lier*, which forms the crest of the hill north-east of Hackness, and a third *out-lier* (not marked in Smith's geological map of Yorkshire) crowning the hill immediately west of Hackness, afford the best possible opportunities for studying the characters of the oolitic formation and its relation to the inferior beds. The same remark may be applied to the fine natural section of Filey Bridge, and to various quarries in the course of the river Derwent where it cuts its way through the oolite into the vale of Pickering. An examination of these localities led me to conclude, without hesitation, that the oolite of this part of Yorkshire was in every respect identical with the middle oolite or coral-rag formation; an opinion which was confirmed by an examination of several quarries near Kirkby Moorside during the following year. As, however, this conclusion is of importance, and has been disputed, I think it expedient briefly to state the evidence on which it is founded.* For this purpose, it will be necessary to determine the true characters of the middle oolite formation in those localities where it is best exhibited.

Section of the Coral-rag Oolite exposed in the Cliffs near Weymouth.

Near Weymouth there are many highly instructive sections, exhibiting all the three formations of oolite, and the intervening clays in their natural order; and in the cliff extending from the jetty to the ruined castle, the successive beds of the coral rag formation (beginning with the lowest and ending with the highest) are exposed in an unbroken natural section. The following details connected with this section are extracted from memoranda made on the spot in the year 1820.

No. 1. A system of beds near the jetty resting immediately on the Oxford clay.† They are composed of a variety of calc-grit, very harsh and meagre to the touch; and many of them are almost made up of irregular stems branching out and intersecting each other in every direction. Heaps of dried branches of wood mixed with blown sand, and then cemented into one mass,

* It is, perhaps, unnecessary to inform the reader that Mr. Greenough, Professor Buckland, and Mr. Smith, have all, in their published works, identified the Pickering oolite with the coral rag formation.

† The Oxford clay near Weymouth is highly characteristic, and contains an incredible number of the *gryphæa dilatata.*

would in some measure approach to the configuration of these singular beds. The stems very seldom exhibit any indications of organic structure, but I think that some of them must have originated in the presence of alcyonia, or of other genera of the lower class of zoophytes.

No. 2. Many thin beds of yellow sand and sandstone, very much resembling the sands of the inferior oolite.

No. 3. Strong beds of calcareous grit in structure like No. 1, but more ferruginous, and separated into prismatic vertical masses by a double system of fissures. The three preceding groups extend from the jetty to the first headland.

No. 4. Blue argillaceous beds alternating with hard compact beds which lose the character of sandstone, and have a nearly even fracture.

No. 5. Beds of yellow sand resembling No. 2. Near the top are beds of calcareous grit with argillaceous partings, in structure resembling the beds of Nos. 1 and 3.

No. 6. Many beds of pure oolite with argillaceous partings, alternating with other shelly oolitic beds, somewhat resembling forest-marble. In some of these beds, the oolitic particles were associated with a variety of marl, and were not coherent.

No. 7. Thin beds of oolitic marl containing innumerable specimens of a small *echinus* (*Clypeus clunicularis*), casts of *melaniæ*, &c.

No. 8. A group of beds of impure sandy oolite. It contains, along with various fossils of the preceding groups, a few specimens of the *ostrea deltoidea.*

No. 9. A large group, in the lowest portion of which are masses of coral rag containing the ramose madrepore (*caryophyllia*), and other fossils of Steeple Ashton, mixed with innumerable fragments of the *trigonia clavellata.* In the higher portion of this group are many meagre sandy beds nearly resembling the calcareous grit of some of the inferior groups of this section; containing, however, more calcareous matter, and a much finer suite of organic remains.

No. 10. Kimmeridge clay, with large beds of the *ostrea deltoidea.*

No. 11. Beds of ferruginous impure calcareous grit, partially oolitic, and alternating with beds of red and green-sand, and blue clay containing the *ostrea deltoidea.**

No. 12. The remaining portion of the cliff on the way towards Portland ferry is composed of Kimmeridge clay, containing the most characteristic fossils of that formation.

* This very remarkable group is seen in the part of the cliff immediately under the ruined castle. The whole thickness of the groups from No. 1 to No. 9 inclusive, must be considerably greater than the thickness of the same formation near Oxford, which is estimated by Mr. Conybeare at something less than 200 feet (p. 185). In some places in Yorkshire, the thickness of the calcareous grit, without the oolite, has been estimated at 200 feet.—("Survey of the Yorkshire Coast," p. 77.)

Any person who had frequent opportunities of access to this part of the coast might select a fine suite of organic remains illustrating the natural history of the formation. The series which fell under my own notice must, of course, be very imperfect; and I only give it in this place in the hope of conveying some notion of the manner in which the fossils are grouped in the several parts of the section just described.

The fossils of the five lowest groups (No. 1 to No. 5) were as follows:*—1. *Ammonites.* 2. *Nautilites.* Both these were rare, and generally in the form of fragments. 3. *Belemnites* ——. 4. *Gryphæa dilatata,* confined to the lowest group No. 1. 5. *Ostrea,* (*a*) a large flat species, (*b*) a small convex species, (*c*) *ostrea Marshii* ——. 6. *Trigonia clavellata.* 7. *Pecten,* two or three species; one small species with undulating striæ is very abundant. 8. *Pinna lanceolata.* (?) 9. Many casts of a shell resembling a *mya* are found in all parts of the formation.

The fossils of the central portion of the formation including Nos. 6, 7, 8, of the general section were as follows:

1. *Ostrea,* (*a*) small convex species; (*b*) *deltoidea,* found in the group No. 8. 2. *Pecten,* the small species abovementioned. 3. *Perna aviculoides.* 4. Many casts of a *mya* (?). 5. Many fragments of various bivalves ——. 6. *Melania,* (*a*) *Headingtoniensis,* (*b*) *striata,* associated with various casts of univalves. 7. *Echinus, clypeus clunicularis.*

Some species of fossils of the group No. 9 are in infinite abundance. Whole beds are entirely made up of crystalline fragments of the *trigonia clavellata* and other characteristic shells. This association of petrified shells and corals with incoherent ferruginous marly beds gives rise to a variety of rubbly limestone, provincially called *coral-rag,* a term now generally adopted by English geologists. The series exhibited in this group is as follows:—1. *Ostrea,* (*a*) *gregaria,* (*b*) *solitaria,* (*c*) *Marshii,?* (*d*) *deltoidea.* 2. *Trigonia,* (*a*) *clavellata,* (*b*) *elongata* (?), (*c*) *costata.* 3. *Pecten,* the small species with undulating *striæ.* 4. *Pinna,* (*a*) *lanceolata,* (*b*) *granulata.* 5. *Perna,* (*a*) *aviculoides,* (*b*) a flat species of a trapezoidal form. 6. *Terebratula inconstans.* 7. *Melania,* (*a*) *Headingtoniensis,* (*b*) *striata.* 8. *Turbo muricatus.* 9. *Trochus reticulatus.* 10. *Turritella muricata.* 11. *Echinus* (*a*) spines and fragments of *cidaris papillata,* (*b*) *clypeus clunicularis* ——. 12. A few fragments of the ramose madrepore (*caryophyllia*), and other coralline bodies resembling those which abound in the well-known quarries of Steeple Ashton, &c. &c. Of the preceding list, No. 1 (*Ostrea deltoidea*), No. 3, No. 4 (*Pinna granulata*),

* The names of the several species of *testacea* are adopted from Sowerby's Mineral Conchology, except where the contrary is expressed.

No. 6, and No. 9, are also found near Weymouth in the next superior formation of Kimmeridge clay.

The group (No. 11 of the Weymouth section) is within the limits of the Kimmeridge clay. The ferruginous beds subordinate to this group contain several fossils, among which are; 1. *Ostrea deltoidea*, and another large flat species. 2. *Lima proboscidea*. 3. *Pecten*, the small species above-mentioned. 4. *Mytilus pectinatus*. 5. Casts of *mya* (?). 6. Many irregular branching stems resembling those in the lower part of the formation. In this bed the stems are frequently hollow, and are mineralized by oxide of iron. By way of conclusion to this enumeration, we may remark; 1. That some fossils are common to the Oxford clay, and the lower part of the coral-rag formation. 2. That the upper part of the coral rag formation alternates with the Kimmeridge clay, and that several fossils are common to the two formations. 3. That several fossils (e. g. one or two species of ostrea, trigonia clavellata, belemnites, pecten (the small species), casts of mya (?), &c. &c.) are common to all the beds of the section above described.

In the cliffs between Weymouth and the Isle of Purbeck, also in many places between Weymouth and Abbotsbury, there are good sections of the coral-rag; and with considerable changes in the character of individual beds, there is, in all the localities, a general agreement in the arrangement of the groups, and an entire coincidence in the suites of the organic remains. In every section we find a group of calcareous grit sand and sandstone at the bottom, beds of oolitic limestone in the middle, and beds of comminuted shells, coral rag, &c. at the top of the formation.

Coral Rag Formation near Steeple Ashton, &c.

The formation is exposed near Steeple Ashton in a succession of quarries; but the denudations are not comparable to those near Weymouth. They enable us, however, to determine that the general arrangement of the groups is the same with that above described. Over the Oxford clay are beds of sand and calcareous grit; these are succeeded by beds of coarse oolite (pisolite of Smith); and the oolite is surmounted by beds of ragstone, containing innumerable stems of corals, and a suite of organic remains nearly identical with the Weymouth series.

Near Calne, in Wiltshire, the denudations are imperfect, but the general arrangement of the formation appears to agree with that which is above stated. In that neighbourhood some of the beds are remarkably modified, though not so much as to destroy the leading characters of the deposit. For example, in some of the escarpments near Bow-wood Park, the calcareous matter is almost wanting, and the calcareous grit passes into a very ferru-

ginous sand, and a sandstone which is occasionally so coarse as to approach a conglomerate. The oolite at Abbotsbury and some other places is hard and indestructible; more generally it is soft, and liable to decomposition on exposure. The oolitic particles in their size, form, and arrangement, are not so regular as in the great oolite and the Portland stone. Indeed some of these characters are so general that a person well acquainted with the English strata would, in most cases, be able to identify mere hand specimens of the coral-rag oolite. Lastly, the upper beds of the formation are sometimes highly ferruginous, and pass, when the fossils are not abundant, into beds nearly resembling those which form the bottom group; this is the case near Abbotsbury.

Formation of Coral-rag near Oxford.

Sufficiently minute details respecting this part of the formation are given by Conybeare,* in the "Outlines of the Geology of England;" and I should have thought it unnecessary to do more than refer to it, had not the arrangement of the groups seemed to contradict that which is given above. It is stated (p. 186), that near Oxford, the calcareo-siliceous grit forms the lowest, the coral rag the middle, and the oolitic freestone the upper part of the deposit. This anomalous arrangement appears to admit of the following explanation. Near Weymouth, and in other localities, shelly beds (exactly resembling the coral rag of the top group of the general section above given) are often associated with the middle part of the formation containing the oolite. Where these beds of broken shells, &c. become of considerable thickness, they constitute a formation of coral rag alternating with, or inferior to, the oolitic freestone. May not, therefore, the coral rag and superincumbent freestone of Headington Hill together represent the central group of the Weymouth and Steeple Ashton sections? The conjecture seems to be confirmed by the appearance of the beds in Headington quarries. In that place the top freestone supports the Kimmeridge clay; and the separation between the two is as well defined as a geometric line. Now the instantaneous passage from one formation to another frequently indicates the absence of certain beds or deposits.† May not then the upper part of the Weymouth section be wanting near Oxford? The supposition seems more probable than the inversion of what (upon evidence already stated) appears to be the common arrangement of the subordinate groups.

* "Outlines of the Geology of England," &c. p. 185—193. In this part of the work, Mr. Conybeare assumes the deposit near Oxford as the type of the formation.

† The converse of this rule is, I think, true. When a number of deposits succeed each other in a fixed order, and the highest beds of one deposit alternate with the lowest beds of the next superior; the alternation indicates the perfect development of the series.

Having thus endeavoured to fix the general characters of the coral rag formation, we may now proceed to examine the nature of the oolitic deposit on the Yorkshire coast and in the vale of Pickering. The lower part of this deposit consists principally of beds of calcareous grit and sandstone, in mineralogical structure, perfectly identical with the lowest groups of some of the sections of the coral rag described above. At Filey Bridge, Scarborough Castle Hill, and other localities, we meet with beds aggregated into irregular branching and imperfectly cylindrical masses, exactly resembling those at Weymouth.* Associated with the same beds are many fossils both in their state of preservation and in their species identical with the fossils in the bottom group of the Weymouth section. Out of these may be enumerated a large flat oyster, *ostrea gregaria, ostrea Marshii* (?), *gryphea dilatata, trigonia clavellata, pecten* (the small species which abounds at Weymouth), and the small echinus (*clypeus clunicularis*), &c. &c.

In every place where the formation is well exhibited, there is, immediately over the calcareous grit, a group of beds of oolitic limestone more or less mixed with imperfectly coherent beds containing comminuted fragments of the *ostrea gregaria, trigonia clavellata, perna aviculoides*, and various other fossils. These beds in mineralogical character are perfectly identical with the coral-rag oolite, and present examples of all its varieties. For very complete details respecting the two preceding groups, I must refer to the "Survey of the Yorkshire Coast," p. 61—79.

The sections near the coast do not in general exhibit any important deposit superior to the preceding group. Near the village of Ayton, there is, however, a fine formation of coral-rag over the oolite. The order of the beds is as follows, beginning with the lowest. 1. Dark-brown shelly beds resembling those associated, at Filey Bridge, with the top beds of the calc-grit, and containing the same fossils. 2. Oolitic freestone, many blocks with imbedded fragments of the *perna aviculoides*. 3. Marl beds with numerous casts of the *melania striata*, surmounted by coral rag containing many crystalline stems of *madrepores*, spines of *echinites* of the division *Cidaris*, &c. &c. This order appeared to be identical with that which is exhibited in the Steeple Ashton section.

In the preceding lists, those species only are enumerated which fell under my own observation, and which may be considered of ordinary occurrence.† The result of the whole exami-

* Some of the branching stems at Filey Bridge and Scarborough exhibit distinct traces of organic structure.

† I have endeavoured, from the only materials in my possession, to convey some notion of the distribution of the organic remains through the several parts of the formation. No one, during a hasty excursion along the coast, has the power of collecting a good suite of fossils. Fine specimens may sometimes be procured from dealers; but these persons collect from all parts of the coast, and sell the specimens without any cor-

nation seems to prove, that in the structure of the component beds, in the arrangement of the groups, in their order of superposition, and in the suites of organic remains, there is an almost perfect coincidence between the Yorkshire oolite and the coral rag formation in the south-western parts of our island. Indeed the coincidence is much more perfect than one commonly discovers in comparing the remote parts of one continuous deposit.*

Sect. 6.—*Coal Formation of the Moorlands, &c.*

With very limited exceptions, the whole cliff extending from the neighbourhood of Filey Bridge to the eastern end of Robin Hood's Bay, and the higher portions of the cliff between Robin Hood's Bay and the extremity of Huntcliff, are composed of beds of shale, sandstone, ironstone, &c. which in their individual character, as well as in their mode of grouping, very nearly resemble the great coal formations of our island. Very elaborate details, connected with the natural history of these beds, their extent, and their range through the interior of the district, are given in the "Geological Survey of the Yorkshire Coast," p. 79—127.

The lowest beds of the group described in the preceding section are succeeded, in the cliffs north of Filey Bridge, and also at Scarborough Castle Hill, by a great bed of shale, which, on first visiting the coast, I considered as the representative of the Oxford-clay. It is, however, in many places subdivided by several beds of sandstone; and in some places the sandstone so far predominates as completely to alter the character of the deposit, which is too much interlaced with the inferior beds to be properly separated from them.† In short, like the shale of the

rect account of the localities. Collections formed in this way tend rather to mislead than to assist in the classification of the successive deposits. In several private collections are many beautiful specimens from the oolite near Malton. Some of these are, perhaps, peculiar to the locality, and several of them are not characteristic of the formation. I remarked among them two beautiful *Echinites*, of the division *Cidaris*, *Melania Headingtoniensis*, and *Melania striata*, *Lima proboscidea*, *Plagiostoma rigidum*, and many other species which abound in the coral-rag of Wiltshire, Dorsetshire, &c. The reader may select a good list of the fossils of the oolite from the plates and descriptions affixed by Messrs. Young and Bird to the "Survey of the Yorkshire Coast."

* Mr. Conybeare thinks it probable that the Yorkshire oolite is the prolongation of the great oolite, which may be traced in one continuous escarpment to the south bank of the Humber; and the map prefixed to the "Outlines of the Geology of England," is coloured on this supposition. The great oolite in Lincolnshire (at least as far as I have examined it) preserves the usual characters, which it exhibits in all parts of its range from the coast of Dorsetshire; and it seems incredible that this great deposit should in the remaining part of its range, through a part of Yorkshire, not only lose these characters, but assume those which belong to a distinct formation. Had I been aware, when I visited the coast, that any question would ever arise respecting the true place of the Yorkshire oolite, I should have visited the formation which extends from Newbald to the north bank of the Humber. A careful comparison of this formation with the oolite of the Vale of Pickering, and with the oolitic terrace of Lincolnshire, would probably settle the question, if the evidence before us be not already considered sufficient for that purpose.

† At Filey Bridge, the great bed of shale makes a well-defined boundary between the

great coal measures, it is not a distinct deposit, but it is a subordinate member of a complex formation.

These beds of shale and sandstone are succeeded (in the cliff between Guisthorpe and White-nab to the south of Scarborough; and also in the cliff extending from the north shore of Scarborough to Cloughton wyke), by a deposit principally composed of beds of sandstone; many of which in their composition and mode of arrangement could not, by the most practised eye, be distinguished from the beds of gritstone which form so considerable a part of the coal formation on the coast of Northumberland. Beds and nodules of ironstone abound in some of the sandstone beds of this series: nodules of ironstone also occur in beds of shale, which alternate with the sandstone. Carbonaceous matter, obviously of vegetable origin, is found associated both with the sandstone and the ironstone; and near Guisthorpe, in the higher part of the series, this matter is aggregated in one or two distinct beds; which are, however, too impure and inconsiderable to be of any value, and are only interesting as affording the first indications of coal in the district.

Notwithstanding the analogies between this system of beds and the true coal measures, a careful examination of many of the extensive sections on the coast would enable us to separate the formations from each other. Thus, near Cayton Mill (about half way between Filey Bridge and Scarborough), a thick bed of oolitic calcareous grit is interstratified with the sandstone; and further north, near White-nab, some shelly beds associated with the sandstone and ironstone, bear no resemblance to any part of the true coal measures. The separation is made still more complete by suites of organic remains, found in various places on the coast, which are identical, not with the older fossils of the great coal formation, but with the fossils common to some of our deposits of oolitic limestone.

The beds of sandstone, shale, and ironstone, which occupy the cliffs for three or four miles to the north of Scarborough, are at Cloughton-wyke succeeded by some beds of meagre grey-coloured calcareous grit, and of hard and nearly compact bluish limestone. These beds, although they are in different places considerably modified both in their texture and arrangement, have been traced (by the authors of the "Survey of the Yorkshire Coast,") through many parts of the interior to the western extremity of the district. Near Cloughton-wyke they contain many obscure traces of organic remains; but make no great feature among the other rocks, and might easily escape notice, were it not for the quantity of stalactitic incrustation which

coral rag and the inferior formation; but in other places where the shale is interlaced with, and almost replaced by sandstone, the same boundary line becomes ill defined and almost arbitrary.

marks their passage through the cliff. They are altogether not more than fifteen or twenty feet thick, and in other places are, perhaps, considerably less; but they require some notice from their peculiar mineralogical character, and from their continuity. They are, however, chiefly interesting from the fact of their forming a separation between the productive and unproductive portion of the peculiar coal formation to which they are subordinate. Traces of bituminous shale may (as has been stated above) be found in certain parts of the formation above these beds of limestone, but no where of sufficient purity and thickness to be of any value.

From Cloughton Wyke to the eastern end of Robin Hood's Bay, the whole coast is composed of alternating beds of sandstone and shale; the greater part of which are, in composition and mode of arrangement, nearly identical with the beds of the great coal formation which are exposed on the coasts of Cumberland and Northumberland. This fact is of importance, and should teach us great caution in identifying distant deposits without first studying every fact connected with their natural history and relations. Almost all the higher parts of the coast, from Robin Hood's Bay to Saltburn, where the cliff terminates in the sandy plain of Redcar, are composed of the lower members of the series resting on the alum-shale; and the same remark may be applied to the structure of the hills which overhang the plain of Cleveland. The sandstone is extremely variable in its colour and structure. It is often iron-shot, and sometimes contains balls and irregular concretionary beds of a variety of clay ironstone. We frequently find in it traces of carbonized vegetable matter, and in some places it contains thin veins and seams of a beautiful coal approaching the state of anthracite. Many of the beds, especially in the lower part of the series, are extensively quarried, and form an excellent strong material for the construction of piers, and for other architectural purposes.

In the sections along the coast, there appears a very peculiar coarse sandstone, provincially called *dogger*, at the bottom of the whole formation, and resting immediately on the alum-shale. This *dogger* in some places appears as a single bed; in other places it is subdivided into several well-defined beds. Generally it is highly ferruginous, and often of a dingy red or ochreous colour: in these cases it is frequently split into distinct blocks by cross fissures, which are coated over with thin ferruginous *laminæ*, and are filled with ochreous clay. Rarely it is of a greenish colour, and then resembles some of the ferruginous sands which have their place under the chalk; not unfrequently it becomes so coarse as almost to pass into a conglomerate form. Most of it effervesces with acids, and it is associated with some

impure calcareous concretions which abound with organic remains.*

The shale in this part of the coal formation not only alternates with the beds of sandstone, but is mixed with them in every possible proportion, and often passes into them, and entirely replaces them. This singular irregularity may be seen in almost every section along the coast, especially in the cliffs south of Whitby. In such circumstances the shale must necessarily be variable in its composition. It sometimes contains balls and nodules of clay ironstone; in some places it becomes bituminous, and contains thin beds of pure coal. The only beds, however, of sufficient thickness and purity to be used, occur near the top and bottom of the series. The higher beds were formerly wrought on the north side of Cloughton Wyke; and beds in the same geological position (i. e. a few yards below the limestone of Cloughton Wyke) have been wrought in several places in the interior of the country. This fact proves that the beds of coal and bituminous shale are not mere local deposits confined to particular troughs or basins. They are interstratified with the other beds, partake of their irregularities, and range with them from the coast to the western limits of the district.†

In the year 1821 a work was commenced in one of the lowest beds at a part of the coast (called Brandy Hole) between Robin Hood's Bay and Whitby. The section exhibited in the cliff was as follows: 1. Alum-shale at the base of the cliff. 2. Ferruginous coarse grit (*dogger*). 3. Alternations of shale and grit 30 or 40 feet. 4. Thick beds of strong coarse sandstone.—Fine specimens of pit-coal had been extracted from one of the shale beds subordinate to No. 3. Similar works in the lower coal beds had been attempted in some of the neighbouring parts of the coast; but I believe they have never been carried on in any part of the interior of the district where the beds are less accessible, and under such circumstances too thin to pay for working.

Such are the leading characters of the great deposit which, on the coast of Yorkshire, is interposed between the alum-shale and the calcareous grit of the coral-rag oolite. As a mere matter of convenience, the upper and lower portions may admit of a separate description; but the whole is so intimately linked together that it must be regarded as one formation, to which there is nothing strictly analogous among the beds of the same age in the south of England.‡

* A small quantity of *galena* was found, by the authors of the "Survey of the Yorkshire Coast," in some slender veins or crevices in the *dogger* near Whitby (see p. 124).

† It is stated in the "Survey of the Yorkshire Coast," (p. 118) that the best seams which have been wrought are about seventeen inches thick, and the worst about six. They are very liable to vary, and are often mixed with bituminous shale; but in favourable instances, they yield a very bright pure bituminous coal.

‡ The whole thickness of the formation is very considerable, but appears to vary in

An examination of the vegetable fossils of the district makes the analogy between the deposit just described, and the great coal formations of England still more complete. Unfortunately I am possessed of hardly one of these fossils, and can, therefore, only speak of them in general terms, without entering on any specific comparison of them with the similar spoils derived from our great coal fields. The most remarkable of these fossils may be divided into the following orders. 1. Ferns. Many species are found in the bituminous shale; beautiful specimens are also sometimes met with in the sandstone beds. A few species are figured in the "Survey of the Yorkshire Coast." (Pl. 2, figs. 4, 5, and Pl. 3, figs. 1, 2.) 2. Arundinaceous plants. Some beautiful upright stems traverse a sandstone bed which overlies one of the lower coal seams near Whitby. 3. Stems surrounded with verrucate impressions, for the attachment of leaves, generally disposed in a quincunx order. Of these, there are many varieties: two are figured in the "Survey of the Yorkshire Coast," Pl. 3, figs. 4, 5.—The condition of these fossils is precisely similar to that of those numerous fossils belonging to analogous orders of the vegetable kingdom in our great coal deposits. On the contrary, there is hardly any thing resembling them in the beds associated with the oolitic series in the south of England.

The organic remains of testaceous animals found in this district do, however, form a link between the Yorkshire beds and the general oolitic series. It is well observed by the authors of the "Survey of the Yorkshire Coast," that these remains much more nearly resemble the fossils of the Pickering oolite than those of the alum-shale. They are very irregularly distributed, in many places entirely wanting, and in general only abundant in those beds where an unusual quantity of calcareous matter is present. In a hasty passage along the coast, I was able to detach very few characteristic specimens. The following imperfect notices respecting the grouping of the fossils are all which I have it in my power to offer from personal observation. 1. In some beds of grit under the great shale bed of Scarborough Castle Hill were, beds of the *gryphæa dilatata, ostrea Marshii* (?), *perna aviculoides,* fragments of a *pinna,* a deeply sulcated *terebratula, serpulæ,* &c. 2. Some calcareous concretions said to be derived from the shale beds north of Filey Bridge, contained *belemnites, ammonites sublævis,* and fragments of other *ammonites, avicula* (*inequivalvis*?). 3. In some beds of sandstone and coarse oolitic calcareous grit near Cayton mill (between Filey Bridge and Scarbo-

different parts of its range. A section described by Messrs. Young and Bird (p. 115), neither reaches the top nor the bottom of the formation, yet passes through beds, the united thickness of which is about 500 feet. Another section (determined by boring) had been proved to a depth of 328 feet below the Cloughton Wyke limestone, without reaching the alum-shale. The greatest thickness of the whole formation is perhaps not less than 700 or 800 feet.

rough) were fragments of a small *madrepore*, fragments of shells, *serpulæ*, a small *ostrea*, traces of *trigonia clavellata*, fragments of *perna aviculoides*, &c. 4. At White-nab, south of Scarborough, are some beds which contain many fossils, among which were remarked, *perna aviculoides, trigonia clavellata* (?), *avicula*, fragments of a *modiola*, &c. &c. 5. Among the beds associated with the limestone of Cloughton Wyke were traces of a *caryophyllia*, resembling the great fossil of the coral rag, traces of a *trigonia*, many casts of a *mya* (?), &c. 6. Among the calcareous nodules derived from the shale beds near the bottom of the formation east of Robin Hood's Bay, I remarked fragments of *ammonites*, a *pecten*, *avicula* (*costata* ?), *mya* (?). Near the same place were beds of small oysters; and in the coarse beds (*dogger*) at the bottom of the formation were *belemnites*, and some other obscure fossils in calcareous concretions; and a *trigonia*, which has sometimes been erroneously named *clavellata*.

These short notices appear to bear out the observations which were made above. Unfortunately I have no opportunity of verifying them; but imperfect as they are, they may convey some general notion of the manner in which the fossils are grouped in this interesting but anomalous formation.*

Sect. 7.—*Alum-shale.*

This immensely thick deposit, chiefly composed of slate clay in different states of induration, begins to rise from beneath the coarse sandstone near the eastern extremity of Robin Hood's Bay. After ascending with great regularity for a few hundred feet, it is interrupted at the Peak by a remarkable dislocation which, on the south-east side of its range through the strata, has produced a subsidence of at least 300 feet, and brought the upper and middle beds of the formation into immediate contact.

At the village of Robin Hood's Bay, the several beds are, by a northern dip, brought down towards the beach; and near Hawsker Bottoms, the highest part of the formation appears once again at the base of the cliff. From thence, in following the line of coast, the strata are observed to make some considerable undulations; but the alum-shale is seen at different elevations in every part of the cliff as far as the south side of the entrance of Whitby Harbour. There the strata are interrupted by a *fault* which ranges up the valley of the Esk, and produces such a *down-cast* on the north side, that the alum-shale is thrown below the level of the beach.† Near Sandsend it again rises, and in

* Some excellent collections of fossils which have been formed by persons resident on the coast, lose a great part of their value from the want of more precise information respecting the localities from which the specimens are derived.

† Similar indications of great *faults*, ranging through our secondary strata in the direction of the principal valleys by which they are intersected, are by no means of rare occurrence. I have observed traces of such *faults* in almost every great valley, which cuts through the magnesian limestone in its range from Nottingham to the mouth of the

the remaining part of the coast as far as Saltburn, it forms a succession of magnificent escarpments, the greatest number of which are, however, crowned by the lowest beds of the superior formation. Lastly, after being concealed for three or four miles by a low cliff of blown sand, it reappears in the reef which runs out into the sea near Redcar.

It would, perhaps, be impossible to give correctly the range of the western boundary of the alum-shale through the interior of the district. From its appearance at Redcar, as well as in the quarries opened in the trap dyke near Langbargh, it is evident that the line passes through the plain at some distance to the west of the great escarpment of the Cleveland hills. In every part of this escarpment, the shale rises to a great elevation; and by its gradual dip towards the south-east is at length carried under the north-western skirt of the Hambleton hills, and disappears altogether, the western boundary being probably buried under the *diluvium* of the plain.

The greatest part of the Yorkshire moorlands north of the oolite hills is composed of the different members of the coal formation described in the preceding section.* The alum-shale is, however, laid bare in the deep valley of the Esk, and in some other similar situations, at a considerable distance from the coast.

For an admirable detailed account of this formation, the reader may consult the "Survey of the Yorkshire Coast," p. 127—160. A very short account of it will be sufficient for my present purpose. The beds which appear on the coast may be conveniently divided into the following groups, beginning with the highest.

(1.) A group, nearly 250 feet thick, composed as follows: —1. Soft glossy unctuous slate clay, 100 feet thick, abounding with pyritous nodules, and extremely liable to spontaneous decomposition. It is from this part exclusively that alum has been manufactured. Its characters may be seen in all

Tyne. *Faults* upon a much more gigantic scale, which have pushed the broken edges of the ruptured slate rocks more than a mile out of their relative position, pass down some of the old valleys which diverge from the central parts of the Cumberland mountains. These facts afford no support to the indefensible theories of De Luc. Breaks and dislocations naturally prepared the way for future valleys; for denuding torrents would more readily scoop valleys out of broken than out of unbroken strata.

* In the colouring of the geological map which accompanies the "Survey of the Yorkshire Coast," the authors have violated their own system; and in the interior of the district have represented a considerable part of the coal formation and alum-shale by one uniform tint. This is unfortunate, and detracts greatly from the value of the map. A geological map ought, for obvious reasons, to represent by appropriate colours those formations only which occupy the surface, without any regard to those formations which are supposed to be below. Had this system been followed, the colour of the coal formation would have been spread over a much larger surface, and the colour of the alum-shale would have been confined to the coast, the western boundary of the district, and the valleys of denudation. The representation of the alum-shale in Smith's geological map of Yorkshire is a good approximation; but there are some errors in it, especially in the representation of the *outliers* near Guisborough.

their details in the cliffs south of Whitby, and in the great alum works at the Peak, Sandsend, Boulby, and Lofthouse. 2. A bed about the same thickness as the preceding, more compact, and in the lower portion containing more nodules and concretions. Near the bottom, the calcareous matter is most abundant, and the beds assume the precise appearance of the *lias formation;* consisting of bands and flattened nodules of argillaceous limestone alternating with seams of shale. Some of these nodules contain *petroleum,* and many when struck give out a strong bituminous odour, which, however, differs from the hepatic odour so commonly given out by fetid limestone. 3. Hard, almost compact slate-clay, about 30 or 40 feet thick, which makes a smooth water-worn cliff, and forms the base of the whole group. The lower parts of this group may be studied with advantage in the cliff north of Robin Hood's Bay, and in the cliff near Runswick Bay.*

(2.) The next group is very well defined, and wherever it appears, makes a prominent feature in the cliff. It is about 120 feet thick, and may be subdivided into the two following portions. 1. Beds of shale, rather meagre and micaceous, alternating with concretions and thin beds of clay ironstone. The concretions of ironstone often contain much calcareous matter, and put on the form of *septaria.* In the lower part of this set of beds, they are least abundant, and only present themselves in the form of a few scattered nodules. 2. Thin beds of micaceous sandstone divided by seams of meagre micaceous slate-clay. The sandstone is in some places siliceous; in others calcareous and shelly, and not unfrequently passes into a rough argillaceous flag-stone. This portion, which is generally more thick and prominent than the one which is superior to it, is well exposed in the cliff north of Robin Hood's Bay, and in the cliff near Staiths.

(3.) The lowest group exposed in the sections of Boulby and Huntcliff, very much resembles the upper part of the first group. It is, however, generally less smooth and laminated, and is not so liable to decomposition. Some portions of this group are sandy; and it is also traversed by some beds of impure shell limestone; the whole thickness exposed in the sections on the coast is above 100 feet.

The united thickness of the exposed parts of these three groups amounts, therefore, almost to 500 feet. And in some fruitless attempts to find coal, nearly 300 feet of still lower beds were formerly pierced through, without reaching the bottom of

* The cliffs composed of the first group are often intersected by a double system of fissures, which divide the shale into rhomboidal masses. This fact is very common in formations of greywacké slate, and indeed in argillaceous deposits of every age. Small crystals of selenite abound in some parts of the decomposing shale. In a vein which intersected the cliff near Runswick Bay, I met with some crystals of selenite of extraordinary beauty.

the formation. The whole thickness of the alum-shale is probably not much less than 1000 feet.

The fossils of the formation are too well known to require any description. It contains ferns and impressions of fish, at least very much resembling those which are found in the lias of the Dorsetshire coast. There are imbedded in it many remains of the *Ichthyosaurus;* and throughout its mass are specimens innumerable of the *gryphæa incurva* and of other characteristic lias fossils. Moreover it does not, I believe, contain a single specimen of the most characteristic fossils, either of the Oxford clay, or of the Kimmeridge clay. If, therefore, strata are to be identified by their imbedded remains, the alum-shale is the true representative of the lias.

Again, the alum-shale in Yorkshire appears precisely in the same situation in which the lias is generally found in its range across our island; viz. at the base of the great terrace which overlooks the plain of the new red sandstone. Lastly, it is grouped in the same manner as the lias. This will be evident by comparing the previous statements with the sections given in the "Outlines of the Geology of England," p. 262, 263. How the conclusion which seems to follow from all these facts is to be avoided, I am at a loss to understand. It may be said that the alum-shale is thicker than the lias formation, and that it contains much less calcareous matter. But the same remark would exclude the shale beds at Lyme, in Dorsetshire, from this formation,—a conclusion which cannot be admitted, inasmuch as the argillaceous cliff of Lyme is surmounted by the inferior oolite and forest marble, and, therefore, cannot be mistaken.*

Sect. 8.—*New Red Sandstone and Magnesian Limestone, &c.*

It is unnecessary in this place to mention the various indications of the new red sandstone formation in the plains of Cleveland, or in the bed of the Tees. In following the line of coast, it first appears near the mouth of the river (a little above West Coatham), in its most characteristic form, associated with veins of gypsum. North of the Tees, it is at Hartlepool succeeded by the magnesian limestone. The sections on the coast do not distinctly show the order of superposition; but the general dip of the beds in the whole district and their relative position, would lead us to conclude that the magnesian limestone was the lowest of the two formations above-mentioned, and that both were inferior to the alum-shale. The fact, however, does not admit of any doubt; for it is well known that the two formations run together to the central parts of the island, and preserve the same constant relations to each other, and to the beds above them.

* Mr. Smith, as has been stated above, identifies the alum-shale with the Oxford clay. In dissenting from such an authority, I cannot refrain from expressing my admiration of the incomparable labours by which he has laid the foundation of all that is accurately known respecting the arrangement of our secondary strata.

Recapitulation.

It appears from what has been stated above, that if we descend from the region of the carboniferous limestone and the coal measures of the county of Durham, cross the plain of Cleveland, and examine the deposits which present themselves along the Yorkshire coast, we shall in succession pass over the following formations:—

1. Magnesian limestone, the lowest of Conybeare's supermedial order.*
2. New red sandstone, and red marl with gypsum.
3. Lias formation. Represented by the *alum-shale,* which in many of its mineralogical characters, in its great subdivisions, in its geological position, and in its fossils, appears identical with the lias.
4. A newer coal formation; but perfectly developed, and immediately succeeded by the coral-rag oolite. This formation, therefore, stands in the place of the whole lower division of the oolite series (viz. inferior oolite, great oolite, forest marble, cornbrash, &c.), and of the Oxford clay.
5. Coral-rag oolite.
6. Kimmeridge clay. The denudations on the coast are here imperfect; but the Portland oolite and Purbeck limestone, the Hastings sands, Weald clay, Shanklin sands, Cambridge and Folkstone galt, and green-sand, appear to be wanting.
7. Chalk formation.
8. Traces of tertiary beds.
9. Diluvium of Holderness, &c.

Such appears to be the result of the facts detailed in the preceding sections of this paper.

Trinity College, Cambridge, April 18, 1826.

P. S. The latter part of the preceding paper was written while the author was absent from the University, and in a place where he had no opportunity of consulting the "Journal of the

* It does not, in this part of England, generally appear in the conglomerate form; but it is separated from the coal measures by a very peculiar formation of sand and sandstone, which is nearly co-extensive with the magnesian limestone, and therefore unconformable to the coal measures. This peculiar formation, which ranges from the southern extremity of Yorkshire into the edge of Northumberland, was first brought into notice by Mr. Smith under the name of the Pontefract-castle rock. In the county of Durham, it often appears in the form of coarse light-coloured, and almost incoherent sand of extremely variable thickness. The most magnificeut exhibition of it is in the banks of the Weare, above the northern boundary of the magnesian limestone. It is there of a very great thickness, and appears partly in the form of yellow incoherent sand, and partly in a form exactly resembling many portions of the new red sandstone. It is not a little extraordinary that this most interesting deposit (which is likely to produce very ruinous effects in shafts sunk for coal within the boundary of the limestone) should still be almost entirely unknown to the practical men resident in the great northern coal-field.

Royal Institution." On that account, he was prevented from referring to Prof. Brande's remarks on the classification of the English formations between the *chalk* and the *red marl* ("Journal of Science," No. 39, p. 29—31). Any thing proceeding from the pen of the Secretary of the Royal Society is entitled to our attention and respect. But the remarks in question appear rather to have been compiled from the authority of others, than to have been the result of personal observations; and it has been shown above, that in the classification of certain formations below the chalk, our best authorities have been at variance. This may account for some statements, in the paper alluded to, which are not borne out by the natural sections exhibited in different parts of our island.

At p. 29 (No. 39), the *blue marl,* which supports the Undercliff rock in the Isle of Wight, is identified with the *Kimmeridge clay* which appears at Shotover Hill, near Oxford. Mr. Smith erroneously identifies the *Weald clay* and *Kimmeridge clay;* other writers have confounded the *blue marl* (Cambridge *galt*) with the *Weald clay.* It is only by a combination of both these errors that the *blue marl* and *Kimmeridge clay* can be confounded. In point of fact, they are separated from each other by five distinct formations.

Mr. Brande states (p. 30), that the formations between the *iron sand* and the chalk "scarcely admit of distinction into strata." The separation of the formations is difficult; but in papers published in the latter part of 1824, in the *Annals of Philosophy,* the difficulties have been completely overcome.

In the subsequent paragraph (p. 30, 31), the sands of Woburn and Ryegate are identified with the sands of Tunbridge Wells and Hastings. In the above-mentioned papers, it is demonstrated that this supposition is erroneous.

At p. 31, it is stated that the *iron sand* "reaches the north coast of Yorkshire, and covers much of its western district." The whole *coal formation,* described in the preceding paper, appears to be confounded by the same author with the *iron sand.* This supposition involves a very great error, and is directly opposed to the evidence exhibited by the sections on the coast.

Lastly, in the same page, the *alum-shale* of Whitby, in Yorkshire, is identified with the *Kimmeridge clay;* an assumption which is entirely inadmissible; for the two formations bear little resemblance to each other, contain a completely distinct suite of organic remains, and have different geological relations.

It has been considered necessary to notice the preceding passages in Prof. Brande's "Outlines of Geology," because they were opposed to the conclusions which this paper was intended to establish.

Article VI.

Reply to Mr. Herapath. By W. G. Horner, Esq

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bath, April* 2, 1826.

On the subject of Mr. Herapath's reply, contained in your last Number, I beg leave to explain, that any charge of *intentional* fallacy was far from my thoughts. Whether I have been guilty of misrepresentation, or advanced a groundless charge, I cheerfully leave to our readers' decision.

I am, yours, &c. W. G. Horner.

Article VII.

On the Accuracy of the Measurement of Heights by One Barometer. By J. Nixon, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Leeds, April* 6, 1826.

Presuming that the comparison of heights measured as well trigonometrically as by *one* barometer might prove interesting to some of your readers, I have taken the liberty to transmit you the following list, computed from observations made in 1822 to 1825 inclusive.

The barometers employed, of the description invented by Sir H. Englefield, were four in number, viz.

One by Troughton, capacity	$\frac{1}{53}$, marked	T in the table.
Dollond	$\frac{1}{94}$	D
Newman	$\frac{1}{45}$	N
Ditto (iron cistern)	$\frac{1}{37}$	I
Unknown..........	$\frac{1}{98}$	U

With regard to the measurements by the first two mentioned instruments, undertaken solely with a view to ascertain that no gross errors had been committed in the trigonometrical observations or calculations, and of which nearly one-half were entrusted to the guides, &c. it must be remarked that little of the attention requisite to insure extreme accuracy was devoted to the generality of the observations. Inapplicable as is the remark to the measurements by the other barometers, which were for purposes of geology, and made with excessive care, it becomes necessary to state, in order to inspire increased confidence in measurements of this description, that there was no *selection* of those favourable circumstances of pressure, temperature, &c. so essen-

tial to complete success. The observations include, on the contrary, not only the extremes of pressure, but also most rapid rates of its variation, and comprehend temperatures ranging, in calms and storms, from 26° to 78° F. Add to which the operations at the reference stations commenced in general much too early in the morning, and were frequently protracted, especially in winter, to a late hour after sunset.

The following methods were resorted to, in order to ascertain the rate of the fall or rise per hour of the barometer, whence to obtain by interpolation the pressure at the base at the instant of the observation on the summit of the mountain. Firstly, by noting the height of the barometer at the lower station before as well as after the ascent; secondly, by repeating the observation of the pressure after a sufficient lapse of time either at the base, or in some eligible situation in its vicinity; and, lastly, by obtaining the pressure at some other low station of which the altitude, as well as that of the one visited before the ascent, had been determined trigonometrically. In one or two instances, the pressure at the summit, when the stay there was considerably protracted, was taken twice; not with the erroneous idea of ascertaining the rate of the variation, but in order to furnish two independent measurements, of which the mean would probably be found more accurate than either separately.

The calculations were made exclusively from the set of tables given in the present volume of the *Annals*. Twice the observed temperature of the air at the upper station being considered as the sum of the detached thermometers, the deduced altitude was finally augmented, as pointed out in Table VI.

With regard to the trigonometrical operations, as I shall probably have occasion to recur to the subject, I restrict myself for the present to the following brief particulars. The horizontal distances were, with few exceptions, derived from the accurate bases given in the third volume of the Ordnance survey; the requisite angles being measured on a good theodolite reading off to 30″. The zenith distances were taken by a most excellent instrument, expressly designed for the measurement of small vertical arcs to the accuracy of 2″ or 3″. On a comparison of the results derived from different bases, &c. it would appear that the error in altitude could never amount to ten feet, and would most probably rarely exceed as many inches.

As the table does not comprise heights surpassing 1700 feet, we may inquire whether the errors, in the case of the measurement by the same method of still loftier mountains, will continue to increase in proportion to the elevation? In the first place, those inaccuracies arising from the impossibility of ascertaining the exact temperature of the mercurial column, which give the appearance of such marked irregularity in the barometrical measurement of small differences of level will be nearly constant

without regard to the altitude. Secondly, the pressure when incorrectly noted at the upper station will introduce errors augmenting in the ratio of the height (or rather inversely as the pressure observed);—errors which increased practice will, however, gradually reduce to insignificant quantities. Thirdly, an improper estimate of the temperature of the air at the summit, although it would vitiate the measurement in the ratio of the height, yet granting the error of observation equal to 3°, its value for an altitude of 10,000 feet would not exceed 65 feet. Lastly, when the decrement of temperature differs from the rate made use of in the construction of Table VI, the errors will increase as the square of the altitude, and may become in some instances a notable proportion of great elevations.

I have the honour to be, Gentlemen,

Your most obedient servant,

J. Nixon.

Heights above the Ground Floor of Gearstones Inn.

			Interval.	Trig.	Error of Bar. +	Error of Bar. −	
Cam Fell	3 miles.	NE	7 hours.	873 ft.	—	38	D.
Cash Knot	3½	ESE	10	882	—	1	D.
Whernside	2½	NNW	8	1362	—	25	I.
Above the Ribble under Horton Bridge.							
Feizer Heights	3	S by W	11	448	20	—	I.
Moughton Fell	1½	SW	11	655	12	—	I.
Dubcote Ridge	2	SE	4	762	8	—	N.
Cam Rakes	5½	NNE	7	903	—	3	N.
Cash Knot	3½	NNE	8	1187	28	—	N.
Ditto	—	—	7	—	—	7	N.
Fountain's Fell	3½	ESE	11	1443	—	12	N.
*Foxhope Fell	3	NE	11	1483	—	61	N.
Ditto	—	—	6	—	—	12	D.
Pen-y-gent	2	ENE	8	1529	—	3	N.
*Ditto	—	—	11	—	—	42	N.
Ditto	—	—	9	—	3	—	T.
Ditto	—	—	6	—	—	32	D.
Above the Wharf under Deepdale Bridge.							
Wharf Head	4	WNW	10	364	20	—	T.
Cam Rakes	3½	W by N	10	743	32	—	T.
Cam Fell	5	WNW	10	1018	—	2	T.
Greenside	3½	NW	6	994	54	—	N.
Deepdale Moor	1	N	4	1090	27	—	N.
Stake Fell	1½	NE	13	1202	—	31	T.
Dod Fell	4½	NW	10	1283	—	33	T.
Ditto	—	—	13	—	—	54	T.

* The temperature at the summits (Oct. 25, 1825) was steadily at 33° from 10 a.m. to 4 p.m.; but on reaching their common *middle* base, 400 feet lower, the thermometer rose on numerous trials to 38°, 39°, and 40°!

Above the Wharf under Hubberholm Bridge.

			Interval.	Trig.	Error of Bar. +	Error of Bar. −	
Raisegill Hag	2½ miles.	W	4 hours.	1220 ft.	26	—	N.
Ditto	—	—	7	—	47	—	D.
Above the Wharf under Buckden Bridge.							
Raisegill Hag	3½	W	11	1256	15	—	N.
Ditto	—	—	6	—	24	—	N.
Litton Hill	3	W	7	1257	8	—	U.
Buckden Birks	1½	WSW	7	1273	—	9	U.
Stake Fell	3	NW	11	1379	30	—	N.
Settron Fell	1½	NE	6	1572	—	12	N.
Ditto	—	—	7	—	48	—	D.
Above the Wharf under Kettlewell Bridge.							
Cover Head	2½	NE	7	950	8	—	N.
Ditto	—	—	6	—	3	—	N.
Ditto	—	—	7	—	—	3	U.
Ditto	—	—	6	—	—	28	D.
Ditto	—	—	4	—	—	47	T.
Starbotton Birks	2½	NW	8	1278	—	8	N.
Ditto	—	—	6	—	—	20	U.
Ditto	—	—	9	—	—	13	U.
Litton Hill	6	NW	8	1320	—	3	N.
Kettlewell Cam	4½	N	6	1389	—	43	D.
Settron Fell	4	NNW	7	1635	—	54	T.
Above the Wharf under Linton Bridge.							
Coalgrove, Grassington Moor	2½	NE	4	752	—	68	D.
Meugher Fell	5	NE	9	1359	—	9	D.
Above the Wharf at Harewood Bridge.							
Wescoe Hill	2½	WNW	7	232	10	—	D.
Helthwaite Hill	1½	NW	8	272	—	29	D.
Ditto	—	—	4	—	18	—	D.
Herebelow Hill	2	N by W	4	318	—	9	D.
Stainburn Chapel Ground	4½	NW	8	363	10	—	D.
Great Almias Cliff	3½	NW	8	618	—	1	D.
Ditto	—	—	9	—	3	—	D.
Little Almias Cliff	6½	NW	9	739	11	—	D.
Above the Ground Floor of the Inn at Kilnsey.							
Malham Tarn	5	W	5	625	—	32	T.
Bordley Grit Hill	2½	SSW	8	634	15	—	N.
Ditto Limestone Hill	2	SW	8	728	4	—	N.
Kilnsey Moor	2	WSW	2	860	—	46	T.
Ditto	—	—	2	—	10	—	T.
Ditto	—	—	9	—	—	12	D.
Great Close Hill	5	W	8	909	5	—	D.
Cam Rakes	12	NW	9	1034	—	25	D.
Bladeyke Moss	10	NW	8	1053	1	—	D.
The High Mark	2½	W	9	1130	—	1	T.
Ditto	—	—	3	—	—	52	T.
Ditto	—	—	9	—	47	—	D.
Ditto	—	—	7	—	—	13	N.

Above the Ground Floor of the Inn at Kilnsey (continued).

			Interval.	Trig.	Error of Bar. +	Error of Bar. −	
Flockrake.	3½ miles.	W	9 hours.	1150 ft.	—	29	T.
———	—	—	7	—	—	15	D.
Meugher Fell.	4½	ENE	8	1273	—	25	T.
Cash Knot	11	NW	11	1318	74	—	D.
———	—	—	9	—	—	32	D.
Raisegill Hag.	8½	NW	10	1370	—	36	T.
Coskoe Moor.	6½	NW	10	1432	—	25	N.
Foxhope Fell.	9	WNW	8	1614	13	—	D.
Pen-y-gent	9	WNW	11	1660	37	—	D.
Above the Canal at Skipton.							
Skibeden Haw	1½	NE	2	488	—	12	D.

Sum of errors ...... + 681 feet.
Ditto.............. − 1067
Mean altitude...... 1066
Mean error .... − 5

Article VIII.

Astronomical Observations, 1826.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

*March 9.	Emersion of Jupiter's first satellite	6h	34′	47″	Mean Time at Bushey.
		6	36	08	Mean Time at Greenwich.
March 16.	Emersion of Jupiter's first satellite	8	27	01	Mean Time at Bushey.
		8	28	22	Mean Time at Greenwich.
March 30.	Emersion of Jupiter's second satellite	10	43	21	Mean Time at Bushey.
		10	44	42	Mean Time at Greenwich.
March 30.	Emersion of Jupiter's first satellite	12	15	20	Mean Time at Bushey.
		12	16	41	Mean Time at Greenwich.

* Jupiter not well defined.

Article IX.

Analysis of a crystallized Compound of Hyponitrous and Sulphuric Acids. By William Henry, MD. FRS. &c.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Manchester, April* 12, 1826.

About three months ago, I was indebted to Mr. Mutrie, an extensive manufacturer of sulphuric acid and other chemical products, at Lloyd-field, near Manchester, for the opportunity of examining a substance, which was found in the leaden pipe that conveys away the foul air from one of the chambers used to condense the acid, formed by burning nitre and foreign sulphur in a separate vessel. The weather was at that time intensely cold, and the product of acid had unaccountably fallen off to a great extent. It was suspected, therefore, that the ventilating pipe was closed with sublimed sulphur; but when examined internally, it was found to have been completely stopped, at an angle where from an horizontal it takes a perpendicular direction, by a crystalline solid not unlike borax in appearance. The portion, which was brought to me, formed a solid mass at the bottom of a bottle, from which it could not be detached without so much force as to destroy the shape of any crystals that might have composed it. When kept for a day or two in a warm room, it assumed a soft or pasty form; and, by standing still longer, a liquid of rather thick consistence, and of the specific gravity 1·831, floated over the more solid part.

The crystalline portion of the mass, from which the liquid had been drained, but which still continued a soft solid, was intensely acid to the taste, and, when handled, stained the fingers like strong nitrous acid. When added to water, a rise of temperature of more than 60° F. was produced, and a violent effervescence took place, accompanied with red fumes resembling those of nitrous gas when escaping into the atmosphere. A similar extrication of gas was observed, on pouring the deliquiated portion of the mass into water. By dissolving a given weight of the solid, in a gas bottle nearly filled with water, gas was collected at the rate of 16·6 cubic inches from 100 grains. Of this, which proved to be nitrous gas of remarkable purity, rather more than one-half was evolved without applying heat, and the remainder issued on heating the solution.

When the crystalline substance was heated alone in a small glass bulb, the stem of which was bent so as to pass under the surface of water in a pneumatic trough, it was found to sustain a temperature of 220° F. for more than an hour without parting with any gas; but at 280°, nitrous gas was evolved. A tem-

perature, however, of 400° did not entirely decompose it; for the liquid which remained, when poured into water, gave abundance of nitrous gas. The proportion of that gas, disengaged by heating the solid salt, exceeded what was evolved from the same quantity by solution in water; 100 grains of the concrete substance affording 19·5 cubic inches. Besides the permanent gas, a vapour was also separated by heat, which was evidently nitrous acid, since it tinged a few drops of water, contained in a small receiver, first green and blue, and then orange.

Having ascertained that the concrete salt contained no fixed base, and that it yielded nothing but sulphuric acid, nitrous acid, and nitrous gas, I proceeded to examine the proportions of those two acids by the following simple method.

One hundred grains of the solid in a soft state were dissolved in water, which nearly filled a small gas bottle; and all the nitrous gas, which the solution was capable of affording, was expelled by heat and collected. The liquid was then added to a considerable quantity of warm distilled water, and solution of pure baryta was cautiously poured in, till the whole of both acids was exactly neutralized. The sulphate of baryta, which was immediately precipitated, after being collected and exposed to a low red heat, weighed 200 grains, equivalent to 68 grains of real sulphuric acid. To the filtered liquid, solution of sulphate of soda was added, and a second product of sulphate of baryta was obtained, weighing, when edulcorated and gently calcined, 20 grains. These are equivalent to 21 grains of nitrite of baryta, and to 7·8 of nitrous acid, under which view we may fairly consider it; because though it be true that the nitrous acid is instantly decomposed when brought into contact with any base, and is resolved into nitric and pernitrous acids, yet the quantity of a base, which neutralizes the newly formed acids, must indicate also the quantity of nitrous acid from which they have resulted.

One hundred grains, therefore, of the crystalline substance, afford

		Grains.
Real sulphuric acid		68·000
Nitrous gas (16·6 cub. in.)	5·273	13·073
Nitrous acid	7·800	
Water		18·927
		100·000

In this case, however, the results of analysis do not give direct information of the nature of the original solid, because the elements of the nitrous compounds are doubtless evolved in a state of arrangement very different from that in which they had previously existed in the solid itself. After considering the

subject under various aspects, it appears to me most probable that the following is the constitution of the solid, the atomic numbers being adapted to the hydrogen scale.

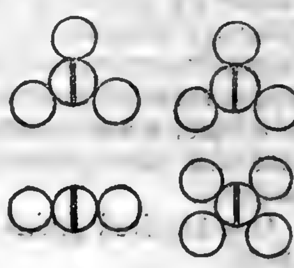
Sulphuric acid, 5 atoms (40 × 5)	200
Hyponitrous acid,* 1 atom.	38
Water, 5 atoms (9 × 5).	45
Weight of its atom	283

Or in 100 parts,

Sulphuric acid..........................	70·67
Hyponitrous acid	13·42
Water..................................	15·91
	100·00

The excess of water obtained by experiment over the theoretical proportion, may be ascribed to the solid having imbibed water in addition to that which is essential to it in a crystallized form.

The changes which the solid undergoes when brought into contact with water, are probably the following:—An atom of hyponitrous acid (which it is convenient for our present purpose to view as constituted of an atom of nitrous gas united with an atom of oxygen) is decomposed; the atom of nitrous gas escapes; and the atom of oxygen, uniting with a contiguous atom of hyponitrous acid, composes an atom of nitrous acid. This will be better understood with the aid of the annexed symbols, representing the atom of nitrogen, and that of oxygen. The two upper symbols denote two atoms of hyponitrous acid, and the two lower ones express an atom of



* The existence of *hyponitrous acid (sub-nitrous acid* of Dalton, *acide pernitreux* of Gay-Lussac), though it has been questioned by one or two chemical philosophers of great name, I consider as established by a variety of facts and arguments, which I have fully stated elsewhere (Elem. of Chem. 10th Edit. i. 312, 318). No doubt on the subject can, it appears to me, exist in the mind of any one who attentively peruses M. Gay-Lussac's excellent memoir, in the first volume of Annales de Chimie et de Physique, on the Compounds of Azote and Oxygen. The series of those compounds, which would otherwise have been an interrupted one, is completed as follows:

	Vols. of nit.		oxy.	Forming.	Atoms of nit.		ox.	Atomic wt. (nit. 14, ox. 8.)
Nitrous oxide (protoxide),	1	+	0·5	.... 1 vol.	1	+	1	 22
Nitrous gas (deutoxide). .	1	+	1·	.... 2 vols.	1	+	2	 30
Hyponitrous acid.......	1	+	1·5	.... 1 vol.	1	+	3	 38
Nitrous acid	1	+	2·	.... 1 vol.	1	+	4	 46
Nitric acid	1	+	2·5	.... 1 vol.	1	+	5	 54

nitrous gas, and an atom of nitrous acid, into which the upper ones are resolved by the transference of an atom of oxygen from the one to the other. To confirm this explanation, it is necessary that the proportion of nitrous gas to the nitrous acid obtained by experiment, should agree nearly with the proportion of the equivalent of the former compound to the equivalent of the latter. Now the proportion of 5·273 to 7·8 approaches so nearly the proportion of 30 (the equivalent of nitrous gas), to 46 (the equivalent of nitrous acid), that we may consider the discrepancy as arising from unavoidable errors of experiment, the fourth proportional being in fact 44·37.

The crystalline solid which has been above described is probably identical with that obtained many years ago by MM. Clèment and Desormes (Ann. de Chimie, lix. 335), by mingling, in a glass balloon, sulphurous acid, nitrous gas, atmospheric air, and aqueous vapour; and also with a similar compound, afterwards formed by M. Gay-Lussac, by adding to sulphuric acid the product of the distillation of nitrate of lead, which he considers as chiefly hyponitrous acid (Ann. de Chim. et de Phys. i. 407). It furnishes another example, in addition to those before known, of a weak acid serving as a base to a more powerful one. The combinations of fluoric acid with silica and with boracic acid, are familiar instances; and M. Berzelius has lately discovered others in the compounds of fluoric acid with the columbic, titanic, tungstic, and molybdic acids. These, however, differ from the compound of hyponitrous and sulphuric acids in possessing greater permanency, so as to form with bases distinct genera of salts, entitled to the names of fluo-titanates, fluo-columbates, &c.; whereas the compound of sulphuric and hyponitrous acids is instantly decomposed by contact with a base, and the salts obtained are identical with those which would have been formed if those acids had been separately united with the same base.

Article X.

An Historical Sketch of Photometry, with Remarks. By the Rev. Baden Powell, MA. FRS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *April* 10, 1826.

To possess some principle upon which to deduce at least tolerably accurate measures of the absolute or relative illuminating powers of different species of natural or artificial light, is of no inconsiderable importance both in respect to many points of scientific investigation, and to various questions of practical and economical calculation.

Under the former head we may enumerate several cases in optics, in which a variation in the intensity of light takes place, which it would be desirable to reduce to some definite measure, so as to establish the law of the phenomenon; for instance, the different degrees of light reflected from a plane surface at different angles of incidence; and the intensities of polarized light reflected at different azimuths of the second glass in Malus's apparatus.

The importance of some mode of comparing the intensity of light from the various natural sources, whether celestial or terrestrial, is also readily seen, towards a more complete knowledge of the constitution of things. To compare in this way the brightness of the fixed stars, as well as of the reflected light of the planets, is of the highest interest to the astronomer; whilst to obtain similar measures of the light emitted from incandescent and burning bodies is not less important to the chemist and the natural philosopher.

In the more mixed departments of physical inquiry, the connexion between the illuminating power and the heating (or, according to my principle, the latent heat of the light), in different cases, readily occurs as a point, to the determination of which the possession of some standard of measurement would be very essential. The phenomena of the solar spectrum are a familiar instance where this sort of measurement is of peculiar interest.

In relation to practical objects, the illuminating powers of the light produced by the different forms of lamps, by the combustion of different materials, and especially of the different sorts of gas, have excited much inquiry and controversy. And in relation to these, as well as the former topics, with all the boasted improvements of science, we have still to confess that no accurate and unexceptionable method of measurement or comparison has yet been proposed, or put in practice.

The interest which both the former scientific questions, and especially these last practical ones, have excited of late, leads us to hope that the subject may, before long, undergo so thorough and well-instituted an examination as may produce some method of obtaining at least an approximate measure of the relative intensities of light.

Meanwhile it may neither be uninteresting in itself, nor, perhaps, without some use in subserviency to such a design, to take a brief and cursory survey of the progress which has been made in this branch of science; and to make a few remarks on the principles of the various methods hitherto proposed, which may possibly lead to some suggestions for their further improvement.

It does not appear that the idea of measuring the intensity of light occurred to philosophers till at a comparatively recent

period. Huygens threw out some hypothetical ideas relative to this sort of inquiry; but it would seem that Marie, a capuchin friar, was the first who treated the subject with any pretension to accuracy; he published a small work about the close of the 17th century, but his first principles were erroneous. (See Leslie on Heat, p. 404.)

Bouguer published his elementary work on the subject in 1729; an improved edition by Lacaille appeared in 1760.

His method is founded on the principle that the tenuity of light varies as the square of the distance from the source; two unequally luminous or illuminated objects are viewed nearly in the same line, and the brighter removed till the impressions appear equal; the relative intensities of light are obtained from the ratio of the squares of the distances. A modification of this method was used by Sir W. Herschel in his experiments on the quantities of light stopped by different media (Philos. Transac. 1800); and in various forms it has been employed in numerous other experimental researches.

Lambert, about 1760, published his treatise, where he introduced the term Photometria. Prof. Leslie censures some of his investigations; while he allows him credit for genius and originality (Leslie, p. 405). He proposed the thermometrical principle; but admitted the difference between its indications and the degree of illuminating power.

Count Rumford proposed a new method by a comparison of shadows; for which an apparatus was contrived, to which he gave the name of a photometer (Phil. Trans. vol. 84). The principle was a modification of that of Bouguer; the shadows being equalized to the eye, the ratio of the squares of the distances of the lights producing them gave the relative intensities. In allusion to methods on these principles, Prof. Leslie observes, that "machinery of such a complex nature is by no means entitled to the name of a photometer; each observation performed by it is really a distinct process of experiment, and which requires dexterity and skill in the operator." (406).

But it is to be observed that all these methods are merely comparative; we have no invariable standard of light, to which other species or degrees of illumination are referred; no absolute point of departure from which a scale of intensity commences.

Mr. Ritchie, in a paper "on Leslie's Photometer, &c." in the Edinburgh Journal of Science, No. 4, maintains that the method of shadows is defective when applied to light of different colours, in which case, he says, it is impossible to bring the shadows to the same degree of density, and that a difference in colour will always be perceptible.

Another modification of the principle of comparing intensities was adopted by Sir W. Herschel, for measuring the relative illu-

minating power of the different primary rays (Phil. Trans. 1800), but it, perhaps, was not susceptible of any great degree of accuracy.

The method employed by M. Frauenhofer in his researches on the spectrum, for the same purpose, consisted in having a diagonal mirror placed at the focus of the eye-tube of a telescope so that its edge bisected the field of view. Its surface being illuminated with variable intensity by a lamp having an adjustment for that purpose, the light was equalized with that of the coloured ray seen in the other half of the field: hence from the squares of the distances of the lamp, the ratio of the intensities was deduced. For a particular description, see Edin. Phil. Jour. No. 19, p. 32.

I will not here enter upon the objections which have been raised to this method in the particular case to which it was applied: they may be seen in an article on Optical Discoveries, in the British Critic, No. I. Quarterly Series, p. 267. But those objections will not apply in general to its application as a photometer for other experimental purposes where the lights do not differ much in colour; the only difficulty in these cases would be, that the absolute intensity of the light of the lamp cannot be depended on as an invariable standard.

Various modifications of the principle of comparing the impression from unequally illuminated surfaces are referred to by several authors. Thus M. Biot mentions the method of comparing the distances at which a book can be read under different illuminations (Traité de Phys. vol. iv. p. 642, note); but all such methods must be allowed to be open to considerable uncertainty.

But we have now to turn our attention to methods of an entirely different kind. The principle suggested by Lambert has been already adverted to: he seemed to take for granted as the basis of his mode of measurement, that the heat *produced by*, or at all events *accompanying* the absorption of light, must be, generally speaking, proportional to the intensity of light acting; but it is far from clear whether he had ascertained how far this heating effect might be considered a property of light, and whether he was aware of any distinction to be drawn between the solar and other sorts of light in respect to the heat accompanying or belonging to them. At the same time (as I have already observed) he clearly admitted the distinction between the *intensity* or *quantity of light*, and the *illuminating power* of a given intensity of any particular light.

In going upon the same thermometrical principle, Prof. Leslie has taken the subject in a more extended point of view: he connected the idea of such a measurement with the theory he considered established from his experiments; according to which the indication of heat would necessarily be a correct measure of the intensity of the rays.

In his work on Heat (1804, p. 160), he mentions as an esta-

blished fact, that if a body be exposed to the sun's rays, it will in every possible case be found to indicate a measure of heat exactly proportional to the quantity of light which it has absorbed, and suggests several experimental methods by which, he says, the exact proportionality may be shown, and that it depends on darkness of colour.

A full account of the principle, construction, and application of his photometer, is given in ch. xix. A short sketch may be acceptable to some readers, especially in reference to the objections presently to be noticed.

This instrument is constructed with a view to measure the degree of heat produced by the absorption of the solar rays, distinct from any other heating influence.

On exposing a delicate blackened thermometer in a glass case to the sun at different elevations, Prof. Leslie found the relative effects correspond exactly with Bouguer's estimate of the intensities of light at the same obliquities (p. 408).

From his former experiments, he conceives it proved in general, that light and heat are convertible; or that heat is only light in a state of combination; and upon this assumption, the absorption of heat by a black surface must be an exact measure of the intensity of light.

The photometer, as is well known, is essentially a differential thermometer, having one bulb of black enamel which absorbs the rays: the other transparent, so that the temperature of the surrounding air does not affect the instrument.

The accumulation of heat in the bulb will of course be modified by the rate of its subsequent dissipation. This is increased by any agitation in the air; hence the whole is enclosed in a thin glass case, which does away this source of inaccuracy (p. 413). The temperature of the black ball continues to rise till the accession of heat is balanced by the loss (p. 426); the effect is unaltered by the size or thickness of the case (p. 435).

Various suggestions for the application of this instrument in different questions of scientific and practical interest, are given in chap. xx.: also in his "Description of Instruments," &c. p. 11.

The merits of this instrument have of late been much canvassed; in particular, Mr. Ritchie, in his paper before referred to (p. 322), has discussed several of the principal objections which may be brought against it; these are briefly the following:—1. The quantity of heat radiated from the black ball will vary with the state of the sky, &c.; the wind will also affect it (by affecting the case). 2. Reflected light interferes with the effect of the sun. 3. *Illuminating power* cannot be measured by it, but only the quantity of light; the former differs with the colour of the light.

In the same paper (note, p. 323), it is stated that Drs. Turner

and Christison found the photometer affected by simple heat: hence Dr. Brewster concludes that no confidence can be placed in the instrument.

In Mr. Ritchie's additional observations (Edinb. Journ. v. p. 104), he ascribes Drs. Turner and Christison's result to using a portable photometer, the heat rising to the top of the case, and thus affecting the upper ball. He also states these further objections; the imperfect and unequal transparency of the case causes the quantity of light admitted to vary; and the light reflected from the inner surface varies at different altitudes of the sun. When the light is vertical (as in the torrid zone), the upper ball will act as a screen to the lower; in this case, also, part of the light falling on the lower bulb is intercepted by that part of it where it joins the stem, and thus excites heat.

These objections appear to me to be well deserving attention. I long since found that in using the instrument great differences appeared in its indications, under different circumstances, which must clearly have been owing to some of these interfering causes.

But there is another source of inaccuracy which deserves peculiar notice. When light is absorbed by the black bulb, the substance of the black glass in the first instance becomes heated, and this before the heat is communicated to the enclosed air: owing to this the glass expands; and thus the first effect is either an apparent contraction of the air, or a very diminished expansion; and at every successive instant, the glass continues to receive more heat, to expand, and then to communicate heat to the air; so that the effect is always retarded until the temperature of the bulb becomes stationary by the balance of radiation, when the temperature of the enclosed air continues to rise for a short time, till the equilibrium takes place in it also. The expansion of the glass being now stationary, as well as the effect on the air, the actual observed effect is the difference of expansion of glass and air; but each of these is proportional to the degree of heat, and consequently their difference is so likewise. Hence results obtained by taking the initial rate of rising with this instrument are not comparable, except when the difference of intensity in the two cases is very small, or nothing. The results obtained by observing the point at which it becomes stationary can alone be relied on.

Prof. Leslie proposed, among other applications of his photometer, its employment to measure the effects of the different rays of the prismatic spectrum; in this case, the distinction is sufficiently obvious that it applies only to the heating, and not to the illuminating power of the rays.

It may be matter of question, how far some of the objections to this instrument, just adverted to, may show its unfitness for giving accurate measures of the kind here proposed; there are,

however, one or two particulars which it certainly will serve to exhibit.

For example, it will serve to show whether there be in the spectrum formed by a prism of any given substance any exterior heat beyond the visible rays: that is to say, any heating powers of a nature transmissible through glass, and acting on the black bulb. It will not measure the *quantity* of such effect, nor determine the point of maximum; for in this case, there is an additional source of fallacy besides those acting in other cases: this is the circumstance that when placed on or near the boundary of the spectrum, so that only a part of the bulb is within it, or that rays of different powers are acting on different parts of the bulb, the balance of absorption and radiation of heat is greater in favour of radiation than in those positions where the whole bulb is equally immersed in the same ray. The effect of expansion in the glass is also different: so that, independently of other sources of error, such measures are not comparable with those in other parts of the spectrum. These circumstances, if not sufficiently attended to, become sources of error to a considerable amount, and in some instances may lead to the most contradictory conclusions.* At all events, in cases where the object is merely to determine the existence of a particular heating effect, not to measure its magnitude, this instrument may be found applicable, or where only a rough estimation is required, or merely to observe whether a considerable increase or decrease of effect takes place, it may, with proper precaution, be found useful.

In Mr. Ritchie's second paper (Edin. Jour. No. 5, p. 107), he gives the method used by Mr. Herschel for obtaining an accurate measure of the heating power of the sun's rays. This method was described in a letter from Mr. H. which formed part of a communication from Mr. R. read before the Royal Society in 1825, but has not, that I am aware, been published any where but in the paper above referred to. It consists in exposing in a glass vessel or large thermometer, a deep-blue liquid for a given time to the direct rays of the sun: noting the increase of temperature, which is purposely rendered but small by properly adjusting the capacity of the instrument: then shading the sun's direct rays, and leaving it exposed for an equal time to the free influence of all other heating and cooling causes, radiation, conduction, wind, &c. and again noting the effect of these. The same difference of these, according to their signs, is the effect of mere solar radiation. Dividing this by the time of exposure, the momentary effect or differential coefficient, which is the true measure of the intensity of radiation, is obtained.

* In particular I allude to some results which have fallen within my own experience. I obtained sometime ago some apparently singular indications respecting the heating power of the solar spectrum, and cannot at present perceive to what source of fallacy they can be traced. I shall probably take a future opportunity of recurring to this subject.

"Availing myself (observes Mr. R.) of the ingenious remark of Mr. Herschel, I have been enabled to obviate the objections to which Mr. Leslie's photometer is obviously liable; and can now employ it with considerable accuracy as a measurer of the sun's radiation" (p. 107).

With the view of, in some measure, obviating the objection against Leslie's photometer, Mr. R. (in a paper read before the Royal Society in 1825) proposed coating the interior of the bulb with a black substance. This improvement, however, is superseded by his own photometer, an account of which is given in the Phil. Trans. 1825, Part I; and some remarks made upon it in the Edin. Jour. of Science, No. 4, p. 339. These remarks are particularly deserving attention, in reference to the practical object of comparing the relative illuminating powers of different species of gas.

This instrument was, in the first instance, invented upon principles deduced from the author's theory, in which, with Prof. Leslie, he supposes light to be merely caloric moving with great velocity; and that heat is developed in black substances by a conversion of light into heat: however, without entering upon theoretical discussion, the construction and action of the instrument are of a nature calculated to avoid the errors just alluded to. It consists of two air-tight chambers connected by a bent tube, containing some coloured liquid, having their outer sides of glass, and within each a diaphragm of black paper. The light admitted through the glass is absorbed by the black paper, and there giving out its latent heat, the temperature of the enclosed air is raised. Two lights placed on opposite sides of the instrument are compared by measuring the respective distances from the instrument necessary to keep the interposed liquid stationary. This instrument possesses extreme sensibility; it is affected by the light of a candle at 20 or 30 feet distance. The author has, however, candidly acknowledged the inapplicability of any such method for comparing the illuminating powers of light of different colour and quality; and in his paper (above quoted), he concludes by saying, that "this celebrated question which has of late agitated not only the philosophical, but even the commercial world, has not yet received a solution sufficiently accurate to command the assent not only of the impartial observer, but even that of rival companies." (p. 325.)

All the varieties of what has been termed the photometer, upon the principle of the heat developed by a black surface absorbing the rays from a luminous body, are founded on the assumption that the heat thus produced is exactly proportional to the intensity of light incident; but although this heating effect has been proved to be associated with the light in the most intimate manner, in a way which I have proved to be strictly analogous to latent heat, it has not been shown by experiment,

nor would it follow on my principle, that any invariable or necessary proportion subsists between the heating and illuminating power; and in some cases the contrary is decisively known to be the case: hence when the term photometer is applied to instruments or apparatus on the principle of comparing the illuminating intensities; in point of fact, a totally different meaning is attached to it. In other words, there is a want of definition as to *what* it is that we intend to measure.

The quantity of light, or its *density,* may, in certain cases, be calculated upon optical principles; but it does not necessarily follow that this bears a proportion to the heating effect. Still less that it is proportional to the illuminating effect on our eyes.

If this last be the point to be measured by a photometer, it is evident that the standard ultimately referred to can be no other than the human eye.

When two lights of different intensity are simultaneously presented to the eye, we are unable, by the greatest degree of experience, to determine with any sort of numerical accuracy the ratio of the illuminating effects; but there are cases in which, as we have already seen, by making the lights undergo certain modifications, an estimate may be formed. Such are the methods proposed for deducing the ratio from the means employed to *equalize* the lights; and of this equalization (at least within certain limits), the eye can very sufficiently judge. Another such principle may be that of diminishing the light till it actually vanishes, or ceases to produce any impression. The different degrees of attenuation necessary may give the ratio of the intensities in different cases. The degree of accuracy to which any such determinations may be carried will depend entirely on the accuracy with which the eye can judge of the equalization of the lights, or the point of evanescence; but the object to be measured is an impression made upon the same organ. It is consequently to be remembered in all such researches that the same uncertainty attaches to the *thing* to be measured, as to the *standard* by which the measure is attempted.

Upon the consideration of the inherent difficulties and uncertainties of the thermometrical method, even in its most unobjectionable form, it seems now to be admitted that we can only look for accurate comparisons on some modification of the principle of equalization of light; but as yet no method combining simplicity and accuracy of application with soundness of principle has been proposed.

With respect to the general principle of equalization, many methods by which it may be effected will obviously occur. The methods hitherto alluded to all go upon the plan of altering the distance of one of the lights, but in some cases other principles may be resorted to; for example, with gas lights it is easy to alter the intensity of one by regulating the supply. This method

was adopted by Dr. Fyfe in his experiments on the illuminating powers of oil and coal gas (Edin. Phil. Journ. No. xxii. p. 367); but he estimated the intensity by comparison of shadows; the objections to which plan have been already adverted to.

Dr. Brewster has proposed a photometer on the principle of viewing a luminous point through a telescope, and drawing in the eye-tube till its image is expanded into a luminous circle. The intensity of light in this circle diminishes as the square of the distance from the point of distinct vision. (Treatise on Phil. Instruments, p. 47, 205), 1813.

The method of comparing different lights suggested by this author is that of thus attenuating the light till the disk becomes invisible, in each case; and then comparing the squares of the distances for the ratio.

In trying this method, the great difficulty I found to be that of being able to say precisely when the object did actually disappear; thus no reliance could be placed on this method for any thing like accuracy.

But Dr. Brewster's principle might, with much greater prospect of success, be applied to a *simultaneous comparison* of two lights. Two similar telescopes might have their eye-tubes fixed in such a position that the extremities next the eye should be close together, and the object ends diverging to the lights, but only forming a small angle. The lights should be distant, and the displacement must take place in the *object* tube, the eye-tube remaining fixed. A piece of ground glass should be placed close to the eye-holes; and the luminous images might be viewed simultaneously as formed upon this; and the equalization thus effected. From some rough trials, I am inclined to think that this method might be found very convenient, and generally applicable; and in its principle it is certainly susceptible of more accuracy than any other of the same kind. A convenient stand and apparatus for fixing the telescopes, and drawing in the tubes, might be readily devised.

One other principle I may here mention, which was originally suggested by Bouguer, and which, perhaps, may be applicable to this sort of measurement. This is the fact that the image of a luminous object, formed by receiving the rays through a minute aperture on a white screen, varies in the intensity of its illumination according to the *size* of the aperture. By contriving a variable aperture, a method might, perhaps, be thus afforded of equalizing two lights; but I have not subjected this idea to trial.

It is confessedly doubtful whether any such methods would be applicable, except in those cases where we have the lights under our command; but in the instance of a comparison of different artificial lights, they may, I conceive, be not undeserving attention; and the difficulty arising from difference of colour in the

flames might be got rid of by receiving the light through similar prisms; and comparing in each spectrum the same coloured ray, which, though it might be neither rigidly homogeneous, nor precisely the same compound in the two cases, would be a sufficiently near approximation to enable the eye to judge of the equalization of intensity; the relative space occupied by the same colour in each spectrum should be measured; and the sum of the relative illuminations multiplied into the relative spaces, will give the ratio of the resulting compound illuminations.

But it must be confessed that to be obliged to have recourse to such troublesome operations and calculations in every measurement will probably make this method inapplicable for general practical purposes. Nor indeed, according to a remark before made, can the name of a photometer be properly applied to an apparatus, in using which so much remains to be done besides obtaining the simple indication. In saying this, I am of course going upon the assumption that difference in colour interferes to an injurious amount with the power of judging of the equalization of lights. But, perhaps, this may not in practice be found so insuperable a difficulty that skill and experience in the operator may not sufficiently overcome it. At all events there are many cases to which this method may be applied without any of the additional precautions here mentioned.

Article XI.

On the Alterations that must necessarily be made in the System of Chemical Mineralogy, in consequence of the Property of Isomorphous Bodies to replace one another in indefinite Proportions. By M. J. Berzelius.*

Ever since chemistry took part in the classification of minerals, and observation of their external characters consequently ceased to be exclusively admitted in the determination of species, the chemical method has met with a difficulty in the property which certain oxides possess of mutually replacing each other, without any accompanying change of crystalline form; whence it happens that when those oxides form colourless compounds, of nearly equal specific gravities, no difference is perceptible in the crystal; it can only be discovered by chemical analysis. Notwithstanding their definition of what constitutes a mineralogical species, the schools of Werner and Haüy have ranged crystals of different composition under the same species; and to avoid this difficulty, Haüy was obliged to admit accidental mixtures, moulded in the form peculiar to a species, by the

* From the Annales de Chimie.

crystallizing power of its constituent parts; but at the very time when the results of accurate chemical analyses began an uncertain contest with that principle of Haüy's school, which considers that *two bodies of different composition can never have the same crystalline form, unless it belong to limited forms* (*formes limites*), the question was all at once decided by the opportune and unexpected discovery of M. Mitscherlich, according to which, bodies composed of different elements, but of an equal number of atoms similarly combined, assume the same crystalline form. MM. Rose, Bonsdorff, and Trolle-Wachtmeister, have already profited by the light which this discovery has thrown on mineralogy, and proved that the species called *pyroxene*, *amphibole*, and *garnet*, contain a great number of different compounds, formed in an analogous manner; so that if a species, according to the received definition, be composed of combinations similar in their elements and in their proportions, the three crystalline forms above-mentioned must contain a great number of mineralogical species; for most of the pyroxenes, amphiboles, and garnets from different localities, differ in the number and proportion of their elements, although those elements are combined in the same manner. However, there certainly is no mineralogist who would not be shocked at the idea of making a particular species of every amphibole or garnet differently composed, and yet, on the other hand, they cannot with any greater propriety be considered as identical. What, therefore, is to be done?

I do not believe that our knowledge is yet sufficiently advanced to enable us to give a satisfactory answer to this question; and hence arises the difficulty of a first attempt to treat mineralogy according to chemical principles. If, on one hand, it be true that two garnets, for instance, having no other common element but silica, cannot be considered as of the same species, it is not less so on the other, that they may differ in an infinity of ways; and, as we must not assume as identical what is not so, nor establish endless varieties, we have to seek between these two extremes that just medium which is by no means easily found. A mean, however, we must adopt, but on the condition of abandoning it for a better, when the further progress of the science shall have enabled us to do so.

It is now clear, therefore, that the hitherto generally received definition of a mineralogical species, *the same elements combined in the same proportions*, whether we add Haüy's expression, *with the same limited crystalline forms*, or not, can no longer be admitted in every case in which isomorphous substitutions interfere; and, till a generally applicable principle shall have been discovered, we must adopt a particular mode of viewing those cases. On one hand, the crystalline form, on the other, the formula of composition mark them as a group of combinations, which, by their greater or less mutual conformity, perfectly

resemble the relation of genus and species in zoological classification. The genus is determined by the chemical formula and the geometrical form; the species by the elements. For greater clearness, let us take an example from the garnet. Its crystalline form is generally known, and the formula of its composition, according to M. Trolle-Wachtmeister (R signifying *radical*), is $R^3 \dddot{Si} + 2 \dddot{R} \dddot{Si}$. These two formulæ determine the genus garnet. M. Wachmeister has proved besides, that $\ddot{R}$ may be lime, magnesia, protoxide of iron, or manganese, either one of them alone, or several, or even all of them together; and that $\dddot{R}$ may be either alumina, or deutoxide of iron, sometimes alone, sometimes combined with each other. Not less, therefore, than eight species, or prototypes of different garnets, must result from these principles, and their mixture produces such numerous varieties that it would be in vain to attempt to distinguish them. I shall give a second example from another species of mineral, in which isomorphism is less frequently met with. According to analysis, chabasie is composed of $CS^2 + 3\ AS^2 + 6\ Aq$, and a small portion of lime which they contain is represented by potash. I have lately analyzed a chabasie, given me under the name of Levyine, in which a small part of the lime was replaced by potash and soda. M. Arfwedson analyzed a Scotch chabasie, in which almost all the lime was replaced by soda and potash. It is clear, therefore, that some chabasies chiefly contain lime, and others soda; that in all, the bases lime, soda, and potash, may replace one another in variable proportions, and thus chabasies from different localities may be differently composed; but nevertheless they retain the same general formulæ of composition. According to M. Beudant, crystals belonging to the rhomboidal system have, in their isomorphous substitutions, angles similar but not absolutely identical (whilst in the regular system it is always the reverse); so that, after having accurately measured the angles of a dolomite, we can determine the relative quantity of lime and of magnesia, in the carbonate of lime and of magnesia, from the measurement of their angles taken separately, which are very similar, but not perfectly equal. If such be also the case with the bisilicate of soda, lime, and potash, in the rhomboidal crystals of chabasie, it is clear that mineralogists, accustomed to measure crystals accurately, will find chabasies with different angles; and it would be as absurd to make distinct species of them, as of dolomites composed of lime and magnesia in different proportions. I presume that the new name of *Levyine*, given to the chabasie which I examined, was derived from a similar circumstance. Here then we have genera, species, and varieties, or if the word genera be objected to, we

have species, subspecies, and varieties. What I have said of garnet and chabasie applies equally to pyroxene, amphibole, mica, &c.

But these ideas cannot be applied to general systematic classification, without producing a deviation from the usual method. Certain general formulæ of chemical composition do not present the same crystalline form; for instance, felspar and albite have the same formula, but not the same crystalline form, and must consequently be considered as more distinct species than two garnets or amphiboles of different composition.

I shall now endeavour to show that the difficulties may, in great measure, be removed, by means of a change in the chemical classification. In a former essay, I demonstrated that the products of the mineral kingdom are best arranged in the order of the electro-chemical relations of their elements, and that they may be placed according to their most electro-positive, or electro-negative principle. Each of these methods has its advantages, and may be equally well employed. In my essay on Chemical Mineralogy, I have grouped the families according to the electro-positive elements, because most of them, in their combinations with the electro-negative elements, impress on them particular characters, which are preserved, more or less perfectly, in all minerals in which they exist; such are lead, copper, cobalt, nickel, iron, barytes, &c.; and since these compounds are often the objects of labours, whose end it is to extract from them a like electro-positive substance, it appeared to me that the facility of applying the science to practice, which results from the circumstance that the combinations of these metals form separate classes, would equal the advantages of the other mode of classification, which, however, is not to be despised, and in which all the metallic sulphurets, for instance, as well as all the silicates, are arranged together. At that time the difficulties arising from the changes between isomorphous bodies were not foreseen. On considering the modifications which that circumstance, now fully established, must introduce in systematic arrangement, it is immediately evident, that where isomorphous changes most frequently occur, classification, if not impossible, must at least be more difficult. The excellent labours of M. Mitscherlich show that these substitutions may take place between electro-negative, as well as electro-positive bodies, without changing the figure of the crystals; but in the combinations presented by the mineral kingdom, frequent changes occur between the most common positive bodies; whilst among the negative, they have only been discovered between the phosphoric and arsenic acids, bodies of rarer occurrence. If negative isomorphous bodies, with sulphur, or silica, were as frequently met with in the mineral kingdom, either classification would present the same difficulties: there

must be less, therefore, in arranging bodies according to their electro-negative principles. However, when we attempt to place compounds that are liable to vary from the isomorphism of their bases, we meet the same difficulties in arranging them together as in the mode of classing them according to the most electro-positive elements; but the difficulties are of less importance. I have shown in my former essay, moreover, not only that the electro-negative disposition has many advantages, but also that all the oxygenated compounds being in that method arranged under oxygen, the first class in mineralogy, that which is purely inorganic, is thereby subdivided into two parts, one containing the oxidated minerals, and the other those which are not oxidated.

In all former systems, including those of Werner and Haüy, attempts have been made to preserve the advantage resulting from a classification according to the positive element. Each metal, properly so called, constitutes in those systems a family which embraces all its combinations. We must renounce this advantage, in a classification according to the negative principle. More than one mineralogist, perhaps, will not like to seek for iron, copper, or silver, in several families in which they are found dispersed. I must, therefore, show how the classification according to the positive element may be adapted to the isomorphous changes. The compounds of garnet, tourmaline, pyroxene, &c. may be placed under several bases just as any metal may be placed under its sulphate, &c.; and thus the names of alum, garnet, or tourmaline, no longer denote mineralogical species, but merely indicate modes of combination; still, however, the exchange of bases, amongst compounds in indefinite proportions, throws some doubt on the place that should be assigned to such or such a garnet; and sometimes the same individual may, with equal propriety, be arranged in more places than one, which always shows a vicious principle in classification. Thus, adopt what method we will, we cannot, if we follow the principle strictly, avoid meeting with something which shocks us by its novelty; but I must add that we are not to reject a thing as false merely because it is new.

It seems then to be demonstrated, first, that in the present state of our knowledge, it is impossible to determine satisfactorily, with regard to minerals in which isomorphous substitution prevails, those that compose mineralogical species; and secondly, that as the interchanges occur chiefly amongst the electro-positive principles of minerals, their classification, according to the most electro-positive principle, cannot be employed without great difficulty.

In a system which classes minerals according to their most electro-negative element, compounds in which isomorphous bases replace one another may be naturally arranged near each other, and it is of less consequence whether we separate the

minerals into different species or not, provided we know what is not perfectly identical, and that, in the particular description of the system, we indicate the limits, and demonstrate that those species may vary without end. When we strictly adhere to electro-negative classification, the compounds, especially in the great families, arrange themselves in so striking a manner in the order of their external characters, that they could not be more completely so classed even according to the system of Werner, in which the analogy of the external characters is the predominant principle; a circumstance which certainly must materially favour the adoption of this method.

I shall now attempt a mineralogical arrangement according to the negative elements, preserving the great division of minerals into two classes, one containing those of inorganic, the other those of organic composition. The first comprehends eighteen families, which succeed each other from the most positive to the most negative, in the following order: iron, copper, bismuth, silver, mercury, palladium, platina, osmium, gold, tellurium, antimony, arsenic, carbon, azote, selenium, sulphur, oxygen, and chlorine. The eight first are composed of only one or two species; but the rest contain a great number, and under oxygen all the oxidated minerals are arranged. It has not appeared to me to be useful or convenient to subdivide these eighteen families, and the distinction between oxidated and non-oxidated bodies follows of course. The place assigned to chlorine, which comes after oxygen, is a deviation from that strict order, which may be defended on the ground that chlorine expels oxygen even from the strongest bases, and is itself expelled by oxygen only from the weakest; and in the acids which it forms, chlorine is positive with respect to oxygen, and consequently should precede it. I have placed it, however, after oxygen, because this last family terminates with salts, and that of chlorine is almost wholly composed of salts. Should iodine be found to belong to the mineral kingdom, I should place it between oxygen and chlorine.

(*To be continued.*)

Article XII.

Analyses of Books.

An Introduction to the Study of the Laws of Chemical Combination and the Atomic Theory, drawn up for the Use of Students. By Edward Turner, MD. FRSE. Lecturer on Chemistry, and Fellow of the Royal College of Physicians, Edinburgh.

The author of this compendium has executed a task for which both the teacher and the tyro are indebted to him. He

justly remarks that the doctrine of which he treats has been neglected from "a confused notion that it is a subject of great difficulty; that it is connected intimately with hypothetical reasoning; and that, consequently, it is neither a necessary object of study, nor of sufficient importance to give a fair return for the labour bestowed in comprehending it."—(Preface, p. iv.)

Dr. Turner has divided his work into six sections; 1. On the laws of chemical combination; 2. Views of Mr. Dalton on the atomic theory; 3. M. Gay Lussac's theory of volumes; 4. Peculiar views of Prof. Berzelius; 5. On the hydracids; 6. Table of atomic weights.

In the first section there are a few statements which call for observation, and we are sure that Dr. Turner will not be displeased if we should offer some remarks, tending, as we trust, to render his work, in a few instances, and only a few, more correct. In the first place (p. 11), in speaking of the compounds of sulphur and oxygen, the oxygen is termed the "variable ingredient." Now we have no objection to this term, provided it mean nothing more than that, assuming sulphur as a standard, the quantity of the oxygen varies in different compounds; but we are apprehensive that the learner might misunderstand the phrase, and not perceive that the sulphur is the variable ingredient, if we were to assume oxygen as the standard.

In p. 19, Dr. Turner alludes to a part of the subject which has been much discussed, and upon which it is extremely difficult to decide satisfactorily; it is, perhaps, even impossible to perform a conclusive experiment; we allude to the action of water upon those chlorides which are soluble in it, and the nature of the changes which they suffer. Dr. Turner asserts with, we think, rather too little hesitation, that when 56 of chloride of calcium are "put into water, one proportion of that fluid is decomposed, and both its elements are employed in reproducing muriate of lime." Now this is, we conceive, begging the question; for we have no proof of the existence of such a salt as muriate of lime; there is no improbability, *à priori*, that chloride of calcium should exist as such in solution; the question is resolvable into two cases; viz. whether when an aqueous solution of muriatic acid is added to an aqueous solution of lime, chloride of calcium and water are formed; or whether when chloride of calcium is dissolved in water, muriate of lime is produced, as Dr. Turner states, and as we believe, to be the case, by the decomposition of water whole question is, therefore, one of probabilities, and that may fairly be assumed to be the true explanation which involves the fewer difficulties.

We have said that our view of the subject coincides with that of Dr. Turner, and as we do not remember to have seen the case put exactly on the grounds which support our opinion, we shall take the opportunity of stating it.

Let us suppose that when chloride of potassium is dissolved in water, an atom of the fluid is at once decomposed, and muriate of potash formed; when tartaric acid is added to the solution, bitartrate of potash is precipitated, and muriatic acid remains in solution. In this case the *decomposition* of water only occurs; there is no *recomposition*, and the question is merely, does the decomposition take place before the addition of the tartaric acid, or is it determined by the presence of tartaric acid, and on account of its affinity for potash? On the other hand, let it be granted that when the aqueous solutions of muriatic acid and potash are mixed to saturation, we have chloride of potassium in solution; on this supposition water must be *formed* by the union of the hydrogen of the acid with the oxygen of the potash; and this supposition is, perhaps, quite as probable as that, when chloride of potassium is added to water that fluid is *decomposed*. Now, however, to the solution of chloride of potassium which has been formed by the composition of water, add solution of tartaric acid, and then water must be decomposed, for potash is formed and bitartrate of potash precipitated. In this case, therefore, when employing perfectly similar ingredients, we must suppose both the composition and decomposition of water, and admit the latter to be effected by predisposing affinity, a doctrine which is now exploded; it appears to us, therefore, that simplicity is greatly in favour of the change which supposes that all chlorides when dissolved in water become muriates, for then only one decomposition occurs in circumstances which require a composition and decomposition, on the idea that chlorides remain such when dissolved in water.

The next observation which we would make is suggested by what we find in p. 20. Dr. Turner considers that the only exception to the regular law of combination is, "when one proportion of one body combines with one proportion and a half, or two proportions and a half of another body." Now, as it is proved, in p. 34, that these compounds form no exception, it would, we think, have been better to have introduced the explanation at once; there is, however, one mode of showing that the existence of sesquisalts forms no exception, and to which Dr. Turner has not adverted, viz. that they are compounds of an atom of a salt composed of one atom of acid and one of base, with one atom of a bisalt.

Dr. Turner has well pointed out a difference between the theory of volumes and atoms: he observes (p. 47), "The theory of volumes has very considerable analogy to Mr. Dalton's law of multiple proportions. The former is indeed, to a certain extent, a consequence of the latter; for if one body unites with another in several proportions, the quantities of the variable ingredient will stand in the same relation to one another, when expressed by volume, as they do by weight. But there is one remarkable

difference. The weights of the two elements of a compound have no apparent dependence on one another. Thus 6 carbon and 8 oxygen form carbonic oxide; 8 oxygen and 14 nitrogen form nitrous oxide; 8 is no multiple by any whole number of 6, nor 14 of 8. But the elements of a compound are always united by volume in the ratio of 1 to 1, 1 to 2, 1 to 3, and so on. This distinction is certainly very obvious; but still there is otherwise such a similarity in the two laws, that the peculiar nature of the ultimate particles of matter which gives rise to the one, must surely be the cause of the other. It is to be hoped, therefore, that the connecting link will soon be supplied, and one fact of great interest has been already determined, which may ultimately be of use in accounting for this difference."

In p. 59, we observe an error of the press which it is very material to correct. The hydrate of potash is stated to be composed of

Potash	48,	whose oxygen is	8
Water	2,	ditto	8

Here 2 is evidently a misprint for 9.

Some good observations are made on Berzelius's law of combinations; these are followed by remarks on the hydracids, a term to which we have a strong dislike, as denoting the existence of an acidifying principle, which we do not admit; it is also, we think, not quite correctly stated, that the radicals (query, radicles) of the hydracids "agree in possessing strong affinity for the pure metallic substances, with most of which they form very distinct and definite compounds;" the radicle of hydrocyanic acid is either azote or carbon, the former does not combine with metals at all, and the union which carbon forms with a few of them can hardly be considered as the result of a strong affinity; and no definite compound of any metal and carbon has as yet, we believe, been discovered.

In concluding these observations, we beg to repeat our recommendation of this work, advising Dr. Turner, in a future edition, to refer to Dr. Wollaston's very curious paper on the Finite Extent of the Atmosphere, for some arguments in support of atomic combination.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 6.—W. C. Milne, Esq. the Rev. Dr. Nicol, and R. Keith Douglas, Esq. were respectively admitted Fellows of the Society; and Mr. Weaver's name ordered to be inserted in its printed lists.

A paper was read, entitled, "Observations made with an Invariable Pendulum at Greenwich, and at Port Bowen in the Arctic Circle; by Lieut. H. Forster, RN. FRS." The ellipticity of the earth deduced from these observations is $\frac{1}{309 \cdot 63}$.

April 13.—R. I. Murchison, Esq. Sec. GS. was admitted a Fellow of the Society; and the following papers were read:

On the Diurnal Variation of the Needle at Port Bowen; by Capt. W. E. Parry, RN. FRS. and Lieut H. Forster, RN. FRS.

The diurnal variation of the needle at Port Bowen is stated to be never less than 1°, and sometimes to amount to 7° or 8°: the observers have recognized a determinate connexion between it and the positions of the sun and moon.

On the Dip of the Needle at different Latitudes between Woolwich and Port Bowen; by Lieut. Forster.

On the Magnetism imparted to Iron by Rotation, at Port Bowen; by the Same: with Remarks by S. H. Christie, Esq. MA. FRS.

April 20.—John Sharpe, Esq. was admitted a Fellow of the Society; and a paper was read, entitled, "A Formula expressing the Decrement of the Law of Human Mortality." By Thomas Young, MD. For. Sec. RS.

PROCEEDINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN, AT THE FRIDAY-EVENING MEETINGS.

April 7.—Mr. Faraday came forward in the lecture-room, and advanced various experiments and arguments in opposition to a generally received opinion, that all solid and fluid bodies in vacuo, or surrounded by gaseous or vaporous mediums, give off, or are surrounded by, a vapour of their own, whatever be their temperature. His object was to show that a limit existed to this production of vapour, and that by diminishing temperature and those causes which favour the production of vapour, not merely was the tension of the vapour diminished, but ultimately such a state attained, that the previously formed vapour would be condensed, or if removed, no further portions would be produced. He first stated Dr. Wollaston's proof of the finite extent of our atmosphere; and considering its state at the extreme limit, concluded, that any change of circumstances by which the elastic

force should there be diminished, (and amongst others, increase of gravity was pointed out,) would cause perfect condensation of that portion of the atmosphere. He then referred to another force, beside gravity, which he considered equally efficient in overcoming the elasticity of an atmosphere reduced to a certain degree of tension, and causing its entire condensation: this was the attraction of cohesion, and many facts and experiments were referred to in illustration of it. These reasonings were then applied to various bodies with which we are acquainted, and the conclusion drawn, that some of these, as silica, alumina, iron, and other bodies, sometimes supposed to exist in the state of vapour in the atmosphere, cannot take up that state under ordinary circumstances, and that other bodies, as zinc, mercury, &c. probably had their limits of vaporization within the range of diminished temperature which we can command.

Mr. Cuthbert exhibited in the library his beautiful American microscope, in which concave mirrors of 0·6 and 0·3 of an inch being used, astonishing magnifying power was obtained, with extreme distinctness, and absence of all colour.

Mr. Parker laid various fine castings of the new alloy, called Mosaic gold, upon the table, for the inspection of the members.

April 14.—Dr. Granville placed the whole of his fine specimens of Egyptian and other mummies upon the lecture-table, and explained to the members the various facts elicited, and conclusions drawn, from an examination of that presented to him by Sir Archibald Edmonston; for an account of which, see vol. ix. of the *Annals*, New Series, p. 462, and vol. xi. p. 215.

April 21.—Dr. Harwood read part of an Essay on the Natural History of the Elephant. It was illustrated by Mr. Deville's enormous cast taken from the head of the late elephant at Exeter Change, by the skull of an African elephant, by various articles used in the capture of elephants, and by numerous smaller preparations, &c. and fine drawings.

A beautiful specimen of modern illuminated writing was laid upon the library table.

ASTRONOMICAL SOCIETY.

March 10.—A paper was read, "On an Appearance hitherto unnoticed in the Nebula of Orion," communicated by the Astronomer Royal. This appearance was detected by means of Mr. Ramage's 25-feet reflector, which is now placed up at the Royal Observatory. It is well known that among a variety of stars, which appear at the same time in the field of view of the telescope with this nebula, there are four very bright ones, which form a trapezium, and at a little distance, three others nearly in a straight line. These three stars, Mr. Pond observes, are neither situated on the edge of the nebula, nor are they parallel to the edge; but they seem to be insulated from the

nebula, the light of which retires from them in a semicircular form, as if they had either absorbed or repelled the light from their immediate vicinity.

The same appearance, the Astronomer Royal remarks, is observable in the trapezium, round the four stars of which the light has also receded analogously, leaving them on a comparatively dark ground. He conjectures that the *stars* have been the immediate cause of the disappearance of the light; and therefore he wishes to draw the attention of astronomers to the phænomenon, as it seems to deserve a marked attention.

The Astronomer Royal has noticed a similar appearance, still more decidedly, in another part of the same nebula, at some minutes' distance from the trapezium.

2. A communication from Colonel Mark Beaufoy, a member of the Council of this Society, was read. It contains,

1st. Observed transits of the moon and of moon-culminating stars, over the middle wire of his transit instrument at Bushey Heath in sidereal time. These were observed in the course of 1825, and amount to 322.

2dly. Occultations of stars by the moon, in number 6.

3dly. Observations of two lunar eclipses in 1825.

4thly. Observations of eclipses of Jupiter's satellites, in 1825, at Bushey Heath. These amount to 25, and the results are given both in Bushey and Greenwich mean time.

There was also read a communication from Major J. A. Hodgson, of the 61st Bengal Native Infantry, Revenue Surveyor-General, residing at Futty Ghur, on the Ganges. This letter records 75 observations of the eclipses of Jupiter's satellites, made at Futty Ghur (latitude 27° 21′ 35″ N.), in the autumn of 1824, and spring of 1825. Some of these observations were made by Major Hodgson himself; and others under his superintendance, by young men who are his apprentices in the Revenue Survey Department. The names of the several observers are given:—each observation has its appropriate meteorological indications registered: and the natures, powers, and qualities, of the telescopes employed, are respectively described. These observations, compared with corresponding observations of the same phænomena in any places whose longitudes have been accurately ascertained, will serve to determine the longitude of Major Hodgson's observatory.

GEOLOGICAL SOCIETY.

March 17.—A paper was read, entitled, "On the Strata of the Plastic Clay Formation exhibited in the Cliffs between Christchurch Head, Hampshire, and Studland Bay, Dorsetshire; by Charles Lyell, Esq. FGS. &c.

The strata of sand and clay which form the subject of this communication are referable exclusively to the Plastic-clay

formation. They occupy an interval in the coast of about 16 miles in extent, between the London clay of Highcliff on the east of Muddiford and the chalk of the Isle of Purbeck. A coloured section of the strata exhibited in these cliffs accompanies the paper. The author first described in detail the cliffs of Christchurch, or Hengistbury Head, which consist of sand and loam, often much charged with bituminous matter, and containing large concretions of ferruginous sandstone and clay ironstone, disposed in fine parallel layers, in which, as well as in the sand and loam, occur black-flint pebbles, lignite, and flattened impressions of fossil trees. Below these strata are dark bituminous clays, alternating with red and brown sands, with occasional layers of black-flint pebbles. After the outcrop of the above strata, the cliffs are low, and about three miles from Muddiford, are composed solely of diluvium. When they rise again in height, their direction corresponds with the line of bearing of the strata, so that the same beds are continuously exposed for eight miles, as far as the mouth of Poole Harbour.

These beds consist of fine white sand, pinkish sand, and thinly laminated argillaceous marles, containing occasionally much vegetable matter; and the whole series exceeding 150 feet in thickness. The section is interrupted for a space of $2\frac{1}{2}$ miles by the mouth of Poole Harbour, and the bars of sand on each side of it. But in the cliffs near Studland, the strata are again seen, consisting principally of yellow and purplish sand, white sand alternating with thinly laminated white clay, and sand with ferruginous concretions passing into sandstone and pipe clay.

The junction of the chalk with the superior strata is very indistinctly exposed, but a thin bed of striated soft chalk-marl rests immediately upon the chalk, as is the case in Alum Bay. The author concludes with observations on the diluvium of this district, composed chiefly of chalk flints, and he infers from its local characters, both here, and in the rest of Hampshire, as well as in the district between the North and South Downs, that it owes its origin, in this part of England, to causes much more local in their operation than those generally assigned. He examines how far the phænomena attending its distribution are consistent with the supposition, that the diluvium was formed in consequence of the protrusion of the inferior through the superior strata, along the anticlinal axis which now separates the tertiary basins of London and Hampshire. Admitting that this elevation took place when all the strata were beneath the level of the sea, Mr. Lyell endeavours to show, that the returning waters, when the land was raised to its present position above the sea, would have strewed the debris of the older over the newer formations, as we now find it; while those of the more recent would not cover, except in inconsiderable quantities, the more ancient strata; and that the marked dissimilarity between

the diluvium of the Wealds of Kent and Sussex, and that of Hampshire and the neighbourhood of London, may thus be accounted for. As the freshwater formations in Hampshire and the Isle of Wight, as well as the Plastic and London clays, are covered by deep beds of a similar gravel, consisting of chalk-flints, the author states several geological facts to prove, that these more recent formations existed when the chalk and tertiary strata were elevated, and, notwithstanding their difference of inclination, even when the strata of Alum Bay assumed their vertical position, and consequently they were all covered indiscriminately by a similar stratum of diluvium.

April 7.—A translation was read of a letter from M. de Gimbernat, of Geneva, principally upon Sulphate of Soda, to G. B. Greenough, Esq. FGS. &c.

A paper, entitled, "On the Geology of the Valley of the St. Laurence;" by John J. Bigsby, MD. FGS. was read in part.

April 21.—The reading of Dr. Bigsby's paper was continued.

E. W. B.

Article XIV.

SCIENTIFIC NOTICES.

Chemistry.

1. *Analysis of the Soot from a Wood Fire.*

The following results were obtained by M. Henri Braconnot, from an examination of the soot from wood:

1. Ulmin, precisely similar to that produced artificially by saw-dust and potash, estimated at	30·20
2. Animalised matter, very soluble in water, and insoluble in alcohol	20·00
3. Carbonate of lime, mixed with some traces of magnesia	14·66
4. Water	12·50
5. Acetate of lime	5·65
6. Sulphate of lime	5·00
7. Acetate of potash	4·10
8. Carbonaceous matter insoluble in alkalies	3·85
9. Ferruginous phosphate of lime	1·50
10. Silica	0·95
11. Acetate of magnesia	0·53
12. Peculiar acrid and bitter principle (asboline) about	0·50
13. Chloride of potassium	0·36
14. Acetate of ammonia, estimated at	0·20
15. Acetate of iron	Trace
	100·00

(Annales de Chimie.)

2. *Analysis of Lamp-black.*

The same chemist has analysed lamp-black, which gave him,

Carbon	79·1
Water	8·0
Resin, similar to that found in a fossil state near London,* and examined by Thomson	5·3
Sulphate of ammonia	3·3
Asphaltum, or bitumen of Judæa	1·7
Sulphate of lime	0·8
Quartzose sand	0·6
Ulmin, about	0·5
Sulphate of potash	0·4
Phosphate of lime, very ferruginous	0·3
Chloride of potassium	Trace
	100·00

M. Braconnot concludes from these analyses, "that all soots contain essentially several sulphates. The presence of a notable quantity of sulphate of ammonia in lamp-black, renders that substance unfit to be used (as has sometimes been done) in the reduction of metals, when it is wished to obtain them pure, and not at all sulphuretted."—(Annales de Chimie.)

Miscellaneous.

3. *Alkaline, digestive Lozenges.*

M. d'Arcet recommends the following formula for the preparation of anti-acid lozenges, for dyspeptic patients, instead of those commonly prepared with magnesia, the frequent use of which, he feared, might lead to the formation of urinary calculi. M. d'Arcet states that he has found great relief from these lozenges in his own case, and that they are now in general request in Paris, Lyons, &c.

℞ Bicarbonate of soda, dry, and in fine powder	ʒi
White sugar, in fine powder	℥iiss
Mucilage of gum tragacanth, prepared with water	q s.
Essential oil of mint (*menthe*) pure, and recently prepared	♏ v.

The bicarbonate of soda and the sugar are put into a very dry bottle, and well mixed by agitation; the mixture is then transferred to a marble mortar or slab, and rubbed up with the mucilage and essential oil, till the whole is thoroughly incorporated into a paste, and then divided into lozenges, which, when dry,

* For an account of the Highgate resin, see *Annals of Philosophy*, vol. ii. p. 9.

should weigh each about 16 grs. As the lozenges are liable to attract moisture, they should be kept in well-closed bottles.

Bicarbonate of soda is used in preference to the carbonate, as less alkaline, and disgusting to the palate, and incapable of injuring the stomach, which the more caustic salt might, perhaps, do.

In proof of the perfect innocence of these lozenges, M. d'Arcet states that the workmen, who pound and sift the carbonate of soda in a manufactory, where it is extracted from the rough material, and where nearly a ton of the salt is produced *per diem*, suffer no injury whatever from it, though, according to his calculation, they must swallow daily, from the fine powder floating in the air of the workshop, at least as much alkali as would be equivalent to 200 lozenges, and one-third of itin the state of carbonate of soda. On inquiring of some of them, who had followed the business for six or seven years, whether they experienced any ill effects from the nature of their employment, they replied, no other than that they were *sooner hungry* and *more hungry* than the workmen in the other parts of the manufactory, who were not exposed to the fine alkaline dust; they said also that their habit was generally rather constipated than relaxed, but that they felt no consequent inconvenience.

M. d'Arcet concludes his note as follows:—"I am persuaded that the alkaline lozenges, prepared with the bicarbonate of soda, are preferable to magnesia lozenges, and the absorbent powders, which have been long employed to neutralize acidity in the stomach; and I hope that their use will procure relief to those numerous persons who, laborious from duty or inclination, allow too short an interval between their meals and studies, and thus impair the functions of the stomach, and too often accelerate the ruin of their health, and consequently that of their moral faculties."—(Annales de Chimie.)

Note.—The proportion of alkali to the sugar in the above prescription appears very small; it may probably be much increased with advantage; as to the quantity of essential oil, it may be increased or diminished, according to the palate of the patient. It may not be useless to observe, that the *bicarbonate* and *carbonate* of soda of chemists answer, respectively, to the *carbonate* and *sub-carbonate* of the English Pharmacopœias.—*Ed.*

Article XV.

METEOROLOGICAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
2d Mon.							
Feb. 1	N E	30·00	29·99	45	40	—	—
2	S W	30·00	29·88	50	41	—	
3	S	30·05	29·89	58	40	—	—
4	S E	30·05	30·05	48	40	—	
5	S W	30·05	29·73	51	43	—	16
6	S W	30·17	29·73	51	40	—	01
7	W	30·51	30·17	47	24	—	
8	S	30·51	30·44	46	23	—	
9	N E	30·44	30·40	45	20	—	
10	E	30·40	30·31	38	29	—	
11	S E	30·31	30·24	45	30	·48	
12	S	30·30	30·24	49	35	—	
13	S W	30·24	30·12	48	39	—	05
14	S	30·12	30·11	47	45	—	17
15	S	30·00	29·92	54	40	—	02
16	S	29·92	29·55	52	45	—	11
17	S W	29·90	29·55	48	32	—	16
18	S W	29·90	29·74	44	32	—	01
19	S W	30·00	29·74	55	42	—	23
20	W	30·46	30·00	45	37	·47	06
21	N W	30·46	30·14	52	40	—	15
22	S W	30·14	30·06	53	42	—	13
23	W	30·32	30·14	50	32	—	—
24	W	30·32	30·16	51	36	—	28
25	W	30·58	30·32	54	37	—	
26	W	30·58	30·46	40	36	—	
27	N W	30·46	30·35	42	36	—	
28	N W	30·35	30·20	55	46	·48	
		30·58	29·55	58	20	1·43	1·54

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Second Month.—1. Drizzly. 2. Fine. 3. Overcast: rainy. 4. Cloudy. 5. Morning fine: afternoon cloudy: night rainy. 6. Cloudy: windy. 7. Very fine day. 8. Morning foggy, with white frost. 9—13. Fine. 14. Drizzly. 15. Ditto. 16. Fine. 17. Showery. 18. Cloudy. 19, 20. Fine. 21. Showery. 22. Fine. 23. Drizzly. 24—28. Fine.

RESULTS.

Winds: NE, 2; E, 1; SE, 2; S, 6; SW, 8; W, 6; NW, 3.

Barometer: Mean height

For the month.................................... 30·147 inches.

Thermometer: Mean height

For the month.................................... 42·589°

Evaporation.................................... 1·43 in.

Rain.................................... 1·54

Laboratory, Stratford, Fourth Month, 24, 1826. R. HOWARD.

METEOROLOGIGAL TABLE.

1826.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.
3d Mon.							
March 1	S W	30·20	29·95	52	48	—	
2	S W	29·95	29·87	55	50	—	36
3	S W	29·87	29·84	50	40	—	15
4	S W	30·06	29·87	56	38	—	15
5	S W	30·30	30·06	42	30	—	
6	S W	30·06	30·02	52	37	—	07
7	S W	30·22	30·02	54	48	·48	05
8	S E	30·30	30·22	52	46	—	02
9	E	30 46	30·30	67	46	—	
10	E	30·48	30·46	70	40	—	
11	S E	30·59	30·48	50	30	—	
12	E	30·59	30·58	42	29	·43	
13	E	30·58	30·18	43	29	—	10
14	S E	30·18	30·05	54	39	—	10
15	N W	30·34	30·05	54	30	—	
16	N	30·55	30·34	50	25	—	
17	N W	30·56	30·41	48	24	—	
18	N W	30·41	30·08	53	29	—	
19	N W	30·14	30·08	50	31	·47	09
20	N W	30·16	30·14	51	30	—	
21	N E	30·14	30·04	44	36	—	
22	N E	30·04	29·83	45	30	—	
23	N E	29·83	29·70	40	33	—	37
24	N E	29·90	29·72	40	34	—	
25	N E	29·93	29·90	46	34	—	
26	N E	30·14	29·93	42	30	—	—
27	N E	30·13	30·02	46	31	·48	
28	W	30·02	29·80	52	42	—	
29	N W	30·21	29·80	50	30	—	
30	N W	30·49	30·21	50	24	—	
31	N W	30·52	30·49	54	27	·33	
		30·59	29·70	70	24	2·19	1·46

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Third Month.—1. Fine. 2, 3. Rainy. 4. Wet morning. 5. Fine. 6. Showery. 7. Rainy. 8. Rainy morning. 9—12. Fine. 13. Fine day: rain at night. 14. Fine. 15. Cloudy and fine. 16—18. Fine. 19. Showery. 20, 21. Fine. 22. Cloudy. 23. Rainy evening: a considerable fall of snow in the night. 24. Cloudy. 25. Cloudy and fine. 26. Fine: a shower of hail at one, p.m. 27—31. Fine.

RESULTS.

Winds: N, 1; NE, 7; E, 4; SE, 3; SW, 7; W, 1; NW, 8.

Barometer: Mean height

For the month.................................... 30·145 inches.

Thermometer: Mean height

For the month.................................... 42·322°

Evaporation.................................... 2·19 in.

Rain.................................... 1·46

Laboratory, Stratford, Fourth Month, 24, 1826. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1826.

Article I.

Experiments on the Colouring Matter of Lac, and on its Application to the Dyeing of Scarlet. By E. S. George, Esq. FLS.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *St. Peter's Hill, Leeds, April* 26, 1826.

Dr. Bancroft was the first to call the attention of the dyers of this country to the use of lac as a colouring matter; his experiments made upon the preparation known as lac lake, appear to have failed in the practical details, yet they led to more successful trials, and lac lake was much employed in the dyeing of scarlet upon the coarser woollen cloths.

But a subsequent improvement in the manufacture in India, imported as lac dye, has furnished us with a dye rivalling cochineal in brilliancy, and surpassing it in permanency; it is to the latter substance our attention has been more particularly directed.

The best kinds of lac dye consist of the colouring matter combined with alumina, and contain a small portion of resin and extractive.

One hundred grains of lac dye marked D. T. boiled two hours and a half in three measured ounces of water, lost 10 grains. The solution was of a light yellow colour, and gave with muriate of tin a plentiful fawn-coloured precipitate. When concentrated, the solution became deep-yellow coloured, and had an intensely bitter taste.

The part insoluble in water was almost entirely dissolved in solution of subborate of soda, or subcarbonate of soda; from these solutions sulphuric acid, not in excess, occasioned a dark red precipitate, muriate of tin a bright red, and sulphate of alumina and potash a full crimson. The supernatant fluid, after the precipitation by sulphate of alumina and potash, was limpid; after that by muriate of tin, peach-coloured; and after sulphuric acid, light-orange.

Ninety-five grains of lac dye were boiled three hours in four measured ounces of water, 10 grains of extractive were dissolved, the remaining 85 grains were digested in solution of subcarbonate of soda, and the solution filtered; on the addition of sulphuric acid a precipitate fell down, which, when dried, weighed 38 grains. It was sparingly soluble in boiling water, and precipitated as the water cooled.

The solution in boiling water had a full peach colour; sulphuric acid first reddened, and then converted it to an orange, muriatic acid very much reddened, nitric acid did not redden so much as the muriatic, supertartrate of potash reddened but not in so high a degree, the acids did not occasion any precipitate, subcarbonate and subborate of soda changed the colour of the solution to a crimson, sulphate of alumina and potash threw down a beautiful dark-crimson precipitate, muriate of tin a bright-red precipitate, sulphate of copper a dull-crimson precipitate, sulphate of iron a dirty-red precipitate.

The colouring matter of lac appears to differ from that of cochineal in its sparing solubility in water; but in the combinations they both form with the metallic oxides and alumina, they closely resemble each other.

Its combinations with the metallic oxides, and the permanency of the colours thus formed, bear a striking analogy to those of extractive; from which it differs in not being precipitated, but rather rendered more soluble by acids, and in its comparative insolubility in water.

Before entering upon the process for dyeing scarlet with lac, we will examine the other substances employed. On the introduction of lac lake, it was found that the resin with which the colouring matter is combined required the action of a powerful acid for its solution; for this purpose, sulphuric acid, or a mixture of the sulphuric and muriatic acids, was employed. The use of so great an excess of sulphuric acid is injurious, not only in impairing the brilliancy of the colour and converting it to a shade too much approaching to orange, but also in rendering the cloths when dyed harsh to the feel; indeed these effects prevented its application except to the coarsest quality of goods. Since the introduction of lac dye, muriatic acid alone has been employed, and it has been found quite sufficient to combine with the alumina, and to dissolve any small portion of resin.

The acid employed is known by dyers as lac spirit, and is formed by dissolving in 60 lbs. of muriatic acid, specific gravity 1·190, 3 lbs. of tin; the solution is colourless and fuming. The solution of tin used scarcely differs from that employed in the dyeing of scarlet with cochineal, except that it contains a larger proportion of oxide of tin.

The aquafortis (nitric acid) should be distilled in glass vessels, and perfectly free from nitrous gas. The aquafortis most

esteemed by dyers is formed of nitric acid specific gravity 1·170, to which 1-20th of muriatic acid specific gravity 1·190, has been added. A notion formerly prevailed amongst practical men that aquafortis should be kept in the carboys twelve months before being used; but if the nitrous gas be carefully separated, no such length of time is required.

To make the solution, 28 lbs. of aquafortis are poured into an earthen vessel, the form of which is slightly conical; this form, by exposing a large surface, allows the gases formed during the solution to escape. A single handful of finely granulated tin is thrown in, and, when dissolved, fresh portions are added (stirring carefully between each addition) until 4 lbs. have been dissolved; the solution, after remaining 12 hours to cool and subside, is ready for use.

It is advisable to use the tartar (supertartrate of potash) in powder.

For the shade of yellow required to form scarlet, young fustic (rhus cotinus) in chips, inclosed in a bag, is used.

The vessels employed for the dyeing of scarlet are of block tin with copper bottoms; some dyers use vessels composed entirely of tin; they are liable to accident from the melting of the tin, if the fire be urged too violently, nor do they wear so long as those of tin and copper. When the copper is kept well cleaned, no deleterious action will occur.

The first operation of dyeing is the mixing of the lac dye; it is performed in an earthen vessel of similar form to that described for making the solution of tin. To each pound of lac dye, of the quality of D. T. (ground to an impalpable powder) three-fourths of a pint of lac spirit are added, and the whole mixed by stirring with a wooden spatula; this proportion will form a very thick paste: four measured ounces of solution of tin for each pound of lac dye are then poured in, and after being again well mixed, the lac dye is left six hours to the action of the acid before being used.

Woollen cloths or yarns before being dyed are well cleaned by fullers' earth and water, which, by removing any oily matter that may adhere to them, and rendering the cloth equally wet, facilitates the deposition of the colouring matter.

For the dyeing of 100 lbs. of pelisse cloth (a broad woollen cloth of thin and open texture), a tin vessel of the capacity of 300 gallons is nearly filled with clear water, and a fire lighted in the furnace; when at the temperature of 150° a dishful of bran and half a pint of solution of tin are thrown in; they, uniting with any impurities floating in the water, form a scum upon the surface, which is removed when the water approaches to boiling.

When boiling, 10½ lbs. of D. T. lac dye previously mixed with seven pints of lac spirit, and three and a half pints of solu-

tion of tin are poured in; a moment after 10½ lbs. of tartar, and 4 lbs. of young fustic in chips, inclosed in a bag, are added, and the whole boiled five minutes. The fire in the furnace is then withdrawn, or smothered, and 20 gallons of cold water poured into the dyeing vessels, 10½ pints of solution of tin are immediately added, and the cloths are entered, turning over the winch during ten minutes rapidly, the fire is then raised, and the cloth turned more slowly; the liquor in the dyeing vessel is made to boil as soon as possible, and the cloths are boiled one hour; they are then carried to the river and well washed, and afterwards washed in a fulling stock with water alone.

These proportions will dye a brilliant scarlet, inclining slightly to the blue tint; if more of an orange shade be required, white Florence argol may be substituted for tartar, and more fustic used.

The cloths described weighed 12 ounces to the yard; heavy stout goods do not require so much of any of the dyeing stuff in proportion; since they are not so easily penetrated by the colouring matter, 10½ lbs. of lac dye will dye 140 lbs. of coating, 24 ounces to the yard.

A scarlet equally brilliant with that dyed in the large way may be produced in small experiments; in the latter case, the proportions are a little different. I have found that for the dyeing of 180 grains of yarn, in a tin vessel of the capacity of six pints, 60 grains of lac dye mixed with 40 grains of lac spirit and 40 grains solution of tin, and in the dyeing vessel 70 grains of tartar, 1 dram (measured) of solution of tin and 12 grains of young fustic, produced a brilliant scarlet.

Lac dye may be substituted for cochineal in most shades of orange, but in the more delicate shades of rose and pink, the large proportion of acid employed to dissolve the lac dye destroys the brilliancy of the colour. I have found in some experiments made with the colouring matter of lac in a state of purity, that all the colours for which cochineal is usually employed may be obtained from it. In lac dye, the colouring matter being combined with alumina, the insolubility of the compound prevents any combination between it and the fibres of wool. Yarn boiled one hour, with a considerable proportion of a lake formed by precipitating the colouring matter of lac from its solution by alumina, scarcely received a stain.

The sulphuric and muriatic acids are employed to dissolve the alumina; the colouring matter being thus rendered soluble unites with the oxide of tin in consequence of superior affinity, and the new compounds combine with the woollen fibre. It is probable that scarlet dye is a compound of tartrate of tin, or tartrate of potash and tin with colouring matter, since it is only on this supposition, that the effect produced on the colour by the quantity or quality of the tartar employed, can be accounted for.

Article II.

On English Books of Naval Architecture. By Mr. Major.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN,

Hoping that a few remarks I have put together on a work just published, entitled, "Papers on Naval Architecture," may not be deemed unacceptable, as tending to give a correct knowledge of the subject, I beg to offer them for insertion in your valuable work. I am, Gentlemen, your humble servant,

John Major.

The subject of naval architecture is now beginning to be studied scientifically in this country; and it is allowed by all persons acquainted with it, that its cultivation must be of material benefit to us. The fact of our being obliged to copy French and Danish ships of war, and even those of America, while they have, in no instance, copied our bottoms, sufficiently shows our inferiority in the art: our merchant shipbuilders also find among the vessels of our rival brethren the best models for imitation; these truths are certainly sufficient to rouse the energies of our nation,—to make them probe the subject,—to inquire why these things are so? The principal reason of this, no doubt, is, the theoretic pursuit of the subject has not been encouraged among us, as is clearly expressed by Mr. J. Knowles, Secretary to the Surveyors of the Navy, in his work on the Dry Rot. His words are, "In proportion that the theoretic construction of ships has been neglected in this country, the practical part has been encouraged." No patronage has been held out in the navy for this object till within a few years. The small extension of it towards the science has already done much: the work under consideration is one of the fruits of it. To use terms of political economy, immediately a demand by encouragement has been made, a supply has been afforded. The proportion of encouragement given to it will determine its future progress on the same principle. The work before us is, therefore, to be received with the pleasing expectations of its being the blossom of a ripe harvest. It is principally on the elementary principles, in which there is little new matter: it speaks, however, in the best terms for itself.

The work is dedicated to Dr. Inman, who is the Professor of the Naval College and School of Naval Architecture. Nobody can regard this gentleman's situation without being struck with its immense importance, and the extent of its duties; he is the only professed teacher of nautical science, of eminence, to a vast navy which commands the ocean, that is paid by government,

Marine colleges, with their professors, abound abroad, and extraordinary exertions are now being made by the French government in nautical science; in this country, we have but one college on a small scale for the purpose, if we may except the Greenwich Asylum: even America, with its numerically inconsiderable martial marine, is on the point of forming an establishment on a large plan, which will give encouragement to the first scientific men to devote their attention to this interesting and highly important branch of knowledge.

The introductory remarks of the Papers on Naval Architecture embrace a cursory view of the present state of the science in this country, as compared with others; and candidly state that the difficulties of the subject have so much impeded its advancement, even in those countries where it is best known, that it is still imperfectly understood. This latter remark applies with too much force to the synthetical composition of a ship, as involving a knowledge of the actions and motions of air and water. Indeed, if we wait for a theory of vessels till those physical problems are mathematically investigated in such a manner as may be applicable to shipbuilding, it is to be feared that we shall never have one. By the analysis and comparison of ships, however, there is confident hope of improvement; much has been done in this way towards perfecting ships, as may be easily seen, by looking at the different performances and qualities of many foreign vessels, as compared with those of our own build.

There is a writer on naval science, Dr. Robison, not mentioned in this little work, but who has probably treated the subjects of the resistance of fluids, of seamanship, and the action of air on surfaces, in a manner as ample and masterly as any writer, in his three articles on them, in the Encyclopœdia Britannica. He expresses himself very plainly on the little improvement, the pure mathematical investigations of the subject from first principles, has contributed to the determination of the forms and equipments of vessels; and, at the same time, states the great improbability, as mentioned above, of much good ever being effected in that channel. In the same article, he recommends the pursuit of the subject, by investigating the results of facts, in their causes and effects. His words are, as may be seen at the commencement of his treatise on seamanship, "But let it be observed, that the theory is defective in one point only; and although this is a most important point, and the errors in it destroy the conclusions of the chief propositions, the reasonings remain in full force, and the *modus operandi* is precisely such as is stated in the theory. The *principles* of the art are, therefore, to be found in these treatises; but false inferences have been drawn by computing from erroneous quantities. The rules and practice of the computation, however, are still beyond controversy. Nay, since the process of investigation is legitimate, we may make use of it in order to discover the very circumstance in which we

are at present mistaken; for by converting the proposition, instead of finding the motions by means of the supposed forces, combined with the known mechanism, we may discover the forces by means of this mechanism, and the observed motions." When it is remembered that Prof. Robison was a midshipman in the navy for seven years, and therefore knew the thing in practice as well as in theory, his observations have additional weight. Mr. Harvey has well expressed similar opinions in the *Annals of Philosophy* for January last.

In the introduction, there is but little historical detail of the progress of the science; and what there is of it consists too much in general reasoning, instead of the production of facts, quotations, and pointed allusions: in writing of such a description, less confidence must be generally placed; and by it less advantage accrues to the science.

A few of the writers on naval architecture are mentioned, but their particular merits are not noticed. Among other treatises on the subject, Chapman's first work on shipbuilding, published in 1775, and translated into the French in 1779, is named; and also the English translation of it from the French by Dr. Inman, whose notes are said to "have greatly enhanced the value of the work." That Dr. Inman deserves much praise for his conduct of the school of naval architecture, there can be no doubt; but that his translation of Chapman, with the notes,* is such a work on the theory of ships of war as can satisfy the present claims of naval science, is not to be affirmed, with a proper view to improvement in this country. The notes do not supply all the additional information of the 50 years between the original and the second translation: besides, the original is on merchant shipbuilding, of peculiar and limited descriptions. If Chapman's large work on ships of the line, published in 1806, at Carlskrona, had been rendered from the Swedish into English, a very considerable difference would then have been made in the relative progress of naval architecture in this country within the last 10 years. Every merchant shipbuilder in the kingdom ought, however, to possess Dr. Inman's translation of Chapman, which is a valuable present offered to them by the Professor.

Chapman is one of the few writers on shipbuilding who adduces facts in support of his observations; and, therefore, the Professor has wisely chosen him as an author. There is not such a display of mathematical attainments in the Swedish author as in most of the French works; but there is more useful matter for shipbuilding than is to be met with in them. The French, in many of their works, appear to have kept the actual consideration of the formation of ships out of their books; continually refining on hydrodynamical and pneumatical principles, they have rather sought the principles of those branches of

* The multiplicity of the gentleman's engagements make it astonishing how he has effected what has been done.

science, than the laws, proportions, and rules of shipbuilding. Clairbois says, "La théorie de l'architecture navale est toute fondée sur des principes de l'hydrostatique et de l'hydronamique," vol. i. p. 210. Now the fact is, that besides the theory of stability, which those pure physical sciences contribute to it, little else has yet been afforded by them to shipbuilding; all the rest is pure induction from facts, which must be compared by mensuration and proportion, the parts of mathematics which are principally useful in shipbuilding. Whatever can be measured or numbered is the department of mathematics to investigate; and as in shipbuilding we cannot advance a step devoid of such calculations, it is impossible to make much progress in it without considerable attainments in geometry, mechanics, and algebra. Much good has been done in this country by the circulation of short rules and methods of computation, from the seminary under Dr. Inman, which enable the practical man to estimate his required proportions, without being in possession of that fund of knowledge, by which the rules were first discovered.

In Chapman's large work, on Ships of War, which is not translated into the French, there is given the proportions of every element in a ship; namely, the total weights of the ship and of its hull, the weights of the guns, the proportional sharpness of the bodies, the surface of sail, &c. These proportions are such as his judgment, applied to his experience, fixed upon as the best; they are, for the most part, expressed by exponential quantities, the indices of which are logarithms: actual trial has determined them all to be good. The principle, which, by uncommon extension, has produced that fine class of vessels, the 60 gun frigates, is not fully developed by him; neither would any of his ships sail with equal celerity to them. The speed of the large frigates, first used by the Americans (though they most likely obtained the draught and equipment from the French, by naval engineers, in the same manner that Washington's army was supplied by military ones),* is, with a good side wind, 13 or 14 knots an hour; while that of the Swedish ships of war, which are all made to sail with one velocity, is only 10½ knots, with a top-sail breeze. The principal table of this book, containing all the proportions and quantities for ships of the line, which M. Carlsund, a pupil of Chapman, did me the favour to translate, is inserted in this article.

Chapman, having discussed the principles of shipbuilding, as they are generally called, in the usual routine, the same exactly as in the leading papers before us, did not, in the larger work, refer again to them. Dr. Inman's translation of them forms a better book for the pupil's perusal than the little work under consideration: there is only one exception to this remark, which

* The common idea that they arose from the alteration, caused by treaty, of line-of-battle ships to frigates, cannot account for their excellent proportions. We have altered seventy-fours, but not with equal effect.

is, that the Professor took no notice of the problem for finding the centre of gravity of the entire ship by experiment, which is of the greatest consequence.

Probably one of the best things that could be done for naval architecture in this country, at the present time, would be to build, in a few instances, from foreign models exactly, using their dimensions, forms, and equipments, altogether. Every body knows, that in England, the subject has not been attended to, in any thing like the degree it has been abroad. Where we have had one good sailing ship, they have had twenty; while ours have been formed by chance, theirs have been determined by principle. Our books on the subject, till lately, are little beyond carpenters' guides.

Where there is confined knowledge, there is always much dread of going out of the beaten path. Many of our English shipbuilders* will not believe, even now, although we have been obliged to copy foreign vessels again and again, that we can with propriety deviate, in the smallest degree, from our customary scantlings. They appear to fancy that additional material and fastening must always produce strength, not considering that the increased weight is the cause of increased momentum of force in rotatory motion, the shocks of which cause separation of the parts; and that it renders the vessels less buoyant and moveable by the waves, thereby adding to their collision. Few mathematical students proceed far in naval architecture without being surprised at the excessive timbering of our English men-of-war, and being convinced of the necessity of altering it. The East India ships are much better proportioned in this respect, and so are all foreign vessels. Our fir ships are of equal sized timber with those of oak, which proves that a diminution might be made in the latter. If the scantlings were reduced, additional timber and substance might be applied to render the topsides impervious to grape shot; and also to deaden the effect of splinters: this perhaps might be done, and yet the weight of the hull of our line-of-battle ships be reduced 200 tons, which would produce a saving of 3000*l.* or 4000*l.* in each ship. The days of the incorrigible errors of prejudice, unenlightened by science, are passed by, and the subject may be treated professionally, with a true regard to principle and improvement.

The second article, which is on the displacement and tonnage of a ship, contains nothing new; we have had it all in our own language these twenty years. Too much nicety, which causes a tedious process, is recommended, in getting the area of the sections, and a proper regard to circumstances is not pointed out. No good and short methods of approximation are given; the analogies of the spheroid and a ship are not tenable. One of the best modes of estimating the displacement is by considering what fraction, multiplied by the product of the midship section

* Mr. Fearnall, an eminent shipbuilder, at Limehouse, is, with several others, an exception to this remark.

and length, will give it, allowing the body to be of a certain sharpness and curvature. In our East India ships of 1325 tons, it is $\frac{5}{6}$ very nearly; for the common 74 gun ship it is $\frac{4}{5}$: in the tables of merchant ships, Chapman gives this ratio for his vessels. The calculations proposed in an article on a Digest of the British Navy, in the *Annals* for November last, would give at once such practical guides. The plank of the bottom of our ships is generally about $\frac{1}{20}$th of the whole displacement.

The consideration of the tonnage is very properly united in this article with that of the displacement. The tonnage, or real burthen of a ship, is the difference of the light and load displacements; or it is the difference of the whole weight of the ship with its contents, and the weight of the hull.

The scale for tonnage, which has long been used abroad, and was printed in Steel's large work on Shipbuilding, 20 years ago, is an ingenious mode of expressing the tonnage of the ship at different draughts of water; and which, when once done for the ship, requires only the trifling correction of observing her light draught of water and displacement at every two or three years' period of service, and making the allowance for the increase or diminution of weight in the scale. As a ship frequently hogs or breaks her sheer, the draught of water at the stem and sternpost are not good guides; in such cases a distance should be taken from the ports in the middle.

The common mode of estimating the register tonnage is not discussed, nor is any computation proposed in its place. Among our ships in general, nothing has retarded their improvement and advance towards perfection, so much as this erroneous rule for estimating our British tonnage.

There can be little doubt, but that great advantage would be derived to the country, by regulations being enforced for the real measurement of the burden of ships. The only objection to a regular calculation of the burthen of a ship is, that it would cost 5*l.* or 10*l.* For effecting it, a plan or draught of the ship must be made, and the lines to which the ship sinks, when there is only her usual portion of ballast in her, and when she is loaded, must be marked; the solid content between the two lines, then being measured in cubic feet, and divided by 35, will show the burthen in tons correctly. Usually, a ship is built from a draught, in which case there is no occasion to measure one off from the body; but in the latter instance, the inconvenience and expense would be very trifling, when compared with the numerous evils attending our present mode. The draughts might easily be attested, and they could be proved at any time. The origin of our common rule, its evils, and other circumstances attending it, would require too long a space to point out in the present article; but, from the consideration I have given it, I am bold to assert, that it has been the cause of the loss of an immense deal of property, and many lives. In the endeavours to evade the

just impost on their commodities, the merchants have sent to sea ships of the very worst description. In the late inquiry on this subject, several good approximate rules were given, of which Dr. Young's was the best: his mode, however, did not include the weight of the ship, and might soon be evaded when burden was the consideration. Whether the duties are levied on the articles, according to their bulk or to their weight, correct calculations should be made of the spaces they occupy, or the depths to which they cause the ship to sink, from an attested and tried delineation of the vessel.

The third article of the work before us is occupied in the consideration of the centre of gravity of the ship. It is by no means copious, including all extant that is valuable; nor is there any thing new in it. Dr. Inman's translation, with the exception before-mentioned, has much more valuable matter on the subject than it contains.

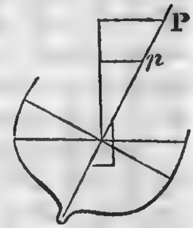
The only methods worthy of consideration for determining the point, are those I inserted in the *Annals of Philosophy* for November last, viz. that of considering the ship as composed of a system of bodies, and finding the common centre of gravity by the usual problem, and the mode of finding it by an experiment on the ship. The latter mode of finding it has generally been attributed to Chapman; whereas Don Juan published it in 1771,* in the Spanish language, and it was translated into the French in 1783, which is four years before it was inserted in the Swedish Academy of Sciences. The proposition may be seen in Don Juan's work, as translated by Levêque, p. 104, vol. ii. Although recommended in 1793 in this country, it has not been undertaken for any ship. In vol. i. art. 900—6, of Don Juan, there are valuable problems applicable to the same purpose. The new solution I gave of the problem coincides in the result with Don Juan's, and therefore it is fair to conclude that it is correct.

In the second new mode of finding the centre of gravity of the ship by experiment, as inserted with that just mentioned in the *Annals* of November last, I perceive I have committed an oversight by taking the inclination to the horizon in one problem, while I had used that from the upright in the former, which is its complement. This rectification applied will make the distance of the centre of gravity of the ship from that of the displacement equal to $\frac{\text{P } a \text{ cosin. } \Delta - b \text{ A}}{\text{P cosin. } \Delta - \text{V sin. } \Delta}$.

There is another mode of finding the centre of gravity of the ship by inclination, which I have not seen noticed by any author. The following is a brief sketch of it. Let the ship be heeled to the same angle by two separate horizontal forces, applied at different heights in the plane of the masts. Then it is evident that their moments of inclining power must be equal,

* This is pointed out by Mr. Read, in No. 11, of a periodical work, entitled, Essays and Gleanings on Naval Architecture; published by Sherwood and Co.

since they are both sustained by the same force of stability. Let P represent one power, and p the other; and let a and b be respectively their distances of action from the centre of gravity of displacement, or any other fixed point; also let Δ be the angle of inclination of the ship from the upright, and put x for the distance of the centre of gravity of the ship from this known point. We shall then have the following equations of their forces, the radius being always 1.



$$P . \overline{a - x} . \cos. \Delta = p \overline{b - x} . \cos. \Delta$$
$$P a - P x = p b - p x$$
$$x = \frac{P a - p b}{P - p}.$$

From the above investigation, we obtain the following

Rule.—Divide the difference of the moments of the two powers from the fixed point, by the difference of the powers: the quotient will be the distance of the centre of gravity of the ship from the fixed point.

The centre of gravity of the displacement had always better be taken for the fixed point, as it will make the signs in the expression more clear.

As I humbly hope some little advantage to naval science may accrue from these remarks, I shall offer more for insertion on the same work. They are in close connexion with the suggestion for a Digest of the Navy, which you did me the favour to insert in the *Annals*, and the use of which, I conceive, I shall be able to make yet more apparent.

In the course of my observations, I have confined myself to truth, as far as I know, which is sacred in science. If any of my ideas clash with those of others, I am sorry for it; but as principles must be discussed, it is impossible to avoid it, and the inconvenience must be borne with: I have subjected myself to the same. Our country can only be maintained in its high political station by the same means it has attained it, among which that of general superiority in the arts and manufactures is most prominent.—Shipbuilding ought not to remain without investigation, and endeavours after improvement, being free from restriction, like the other arts.

The fourth article of the Papers on Naval Architecture is a most excellent and comprehensive disquisition, for a short one, on the stowage of ships; and will be particularly noticed in some future remarks.

It has been stated, that we must not proceed far in our discussion on Naval Architecture, without having recourse to facts and practical observations of ships: for this reason, Chapman's celebrated Table of Ships of the Line will be gladly received, which was before spoken of; it is the most valuable document on shipbuilding.

Particular Elements for the Construction of Ships of the Line, by F. H. Chapman, from his Work in the Swedish Language, on the Proportion of Ships of War, Table No. 33, page 82, which has not been translated into any Language.

Nature of the elements.	Swedish feet. 110 guns.	94 guns.	80 guns.	74 guns.	66 guns.
Displacement to the outside of timbers = D.	152875	128297	107400	96422	88722
Length on the construction water-line = $l = 5{\cdot}18454\ D^{0{\cdot}3088}$	206·84	195·9	185·48	179·40	174·8
Addition on the ends = $\frac{l}{83}$ = f.	2·49	2·36	2·23	2·16	2·10
Of this is added before = $\frac{7}{10}$ f.	1·74	1·65	1·56	1·51	1·47
And abaft $\frac{3}{10}$ f.	0·75	0·708	0·669	0·648	0·63
Whole length of water-line between the rabbits = L.	209·3	198·3	187·7	181·5	176·9
Main breadth on water-line for three deckers = $\frac{l^{0{\cdot}9947}}{3{\cdot}5734}$. For ships of two decks $\frac{l^{0{\cdot}8392}}{1{\cdot}5728}$ = B	56·2	53·3	50·92	49·5	48·46
$\frac{D}{l\,B} = t$.	13·13	12·28	11·37	10·85	10·47
$1{\cdot}6303\ t^{0{\cdot}935} = tr$.	18·11	17·00	15·82	15·15	14·65
Exponent of the parabolic curve, which expresses the areas of the transverse sections; (the less it is, the sharper is the ship fore and aft) = $\frac{t}{tr - t} = n$.	2·638	2·597	2·551	2·524	2·503
Main section area to the outside of the timbers = $B\ tr = \phi$	1019·2	906·9	806	750·4	710
Construction depth = $2{\cdot}37402\ tr^{0{\cdot}7647} = d$	21·75	20·72	19·26	18·98	18·49

A Swedish lineal foot = ·974 English foot.
A Swedish square foot = 0·951 English square foot.
A Swedish cubic foot = 0·927 English cubic foot.

Continuation of Chapman's Table for Ships of the Line.

Nature of the elements.	Swedish feet. 110 guns.	94 guns.	80 guns.	74 guns.	66 guns.
ϕDepth to the upper edge of rabbit of the keel $= 1{\cdot}503\ d^{0{\cdot}87} = q$	21·9	20·98	20·02	19·46	19·03
ϕFrame exponent $= \dfrac{\phi}{B\,d - \phi} = m$	4·976	4·571	4·175	3·964	3·813
Floatation $\frac{1}{2}$ area moulded $= \dfrac{\frac{1}{2}\overline{B\,.\,L}^{1{\cdot}046}}{1{\cdot}7186} = W$	5113·1	4567·2	4109·5	3854·1	3668·6
Floatation exponent $= \dfrac{W}{\frac{1}{2}BL - W} = r$	6·5882	6·3477	6·1372	6·0154	5·9257
Moment of stability for three-deckers in cubic feet of water $= \dfrac{\frac{1}{2}\overline{B^3 L}^{1{\cdot}025}}{2{\cdot}9902}$, for two-deckers $\dfrac{\frac{1}{2}\overline{B^3 L}^{1{\cdot}0714}}{5{\cdot}9551} = \int t \frac{2}{3} y^3 \dot{x} = p\,D$	2·320·500	1·859·900	1·512·300	1·33340	1·210800
From centre of displacement to metacentre, or the $\int t.\, \frac{2}{3} \frac{y^3 \dot{x}}{D} = p$	15·18	14·50	14·08	13·83	13·65
Exponent of displacement from the water-line to the keel $= \dfrac{\frac{1}{2}D}{d\,W - \frac{1}{2}D} = s$	2·198	2·1015	1·9942	1·9332	1·8879
Centre of gravity of displacement from water-line $\dfrac{\overline{s+1}\,.\,\overline{2s+1} + s\,.\,\overline{2s+4}}{2\,.\,\overline{2s+1}\,.\,\overline{2s+4}} = g$	8·572	8·105	7·598	7·308	7·091
Metacentre above water-line $= p - g = S$	6·608	6·395	6·48	6·52	6·559
Common centre of gravity of ship above water-line $v =$	2·80	2·38	2·22	2·23	2·24
Distance of metacentre above centre of gravity $a =$	3·80	4·01	4·26	4·29	4·319
When all the ships with same sails set shall have the same stability, then $\ddot{a}$ shall be equal	3·809	3·975	4·131	4·291	4·348
I have supposed that the centre of gravity shall be before the middle of the water-line L, $\dfrac{L}{76} =$	2·75	2·61	2·47	2·39	2·33

Continuation of preceding Table.

Nature of the elements.	Swedish feet. 110 guns.	94 guns.	80 guns.	74 guns.	66 guns.
Middle of the water-line l is abaft the middle of the water-line $L = 0{\cdot}2$ f. =	0·50	0·47	0·45	0·43	0·42
And then the centre of gravity is before the middle of the water-line $l = a$	3·25	3·08	2·92	2·82	2·75
ϕ Before the centre of gravity $a\,.\,\overline{n+1}$ —	11·83	11·08	10·37	9·94	9·63
Place of ϕ before the middle of the water-line $l = a\,.\,\overline{n+2}$	15·08	14·16	13·29	12·76	12·38
From the abaft end of the water-line l to $\phi = P$	118·5	112·1	106·0	102·4	99·8
Distance, for design, between the sections abaft $\phi = \frac{P}{10}$	11·85	11·21	10·6	10·24	9·98
From the fore end of the water-line l to $\phi = Q$	88·34	83·81	79·45	76·94	75·04
Distance of sections before $\phi = \frac{Q}{10}$	8·83	8·38	7·94	7·69	7·50
Height of lower port sill above water	6·48	6·50	6·92	6·83	6·75
	Cubic feet of water Swedish.				
Weight of guns, balls, wads, powder, carriages	22863	18212	14685	12662	11536
Gunner stores, $\frac{3}{100}$ of above to add.					
Ballast in cubic feet of water	18179	14193	11146	9627	8762
Number of men	1000	848	706	658	606
Hull, rigging, boats, anchors, &c. or the whole ship without guns, stores, ballast, and provisions	86255	73255	61910	55848	51588

This table contains the results of Chapman's determinations for ships of war from his large and expensive work; and therefore is by far the most valuable information contained in it. A copy of the original work is in possession of Mr. J. Knowles, Secretary at the Navy Office, whose scientific exertions for the navy are well known; and whose liberal and obliging conduct in lending his books on nautical subjects, of which he has purchased a large collection, is to be highly appreciated: the more so, as the naval college does not contain within its walls a library on the subject. Previous to my going to Paris, and purchasing such works, I was much indebted to that gentleman for the loan of his books.

Article III.

*On the Use of continued Fractions with unrestricted Numerators in Summation of Series.** By W. G. Horner, Esq.

(To the Editors of the *Annals of Philosophy.*)

GENTLEMEN, *Bath, April* 24, 1826.

1. While the labours of Lagrange have reduced the theory of continued fractions, with the constant numerator 1, to a state little short of perfection, scarcely any thing of consequence, to the best of my knowledge, has been done to render these fractions in their *unrestricted* form available to any useful purpose. The object, however, is not undeserving of attention, since the fractions of Lagrange are applicable almost solely to *numerical* solution; while the others apply even to series in their literal or algebraic form.

If these fractions have not all the clearness of convergency which Lagrange's possess, yet where these are not attainable, they are much preferable to expressions in a finite fraction, on account of the facilities they afford for simplifying the reductions.

The formulæ of Brouncker, who started the discovery of this calculus, have been introduced by Euler, in his *Analysis Infinitorum,* in elucidation of a general method of turning any given series with alternate signs into the form of a continued fraction. The first of Euler's formulæ is contained in the following proposition, and when aided by a reduction that will presently be described, it is adequate to all the others.

* Substituted for the investigation mentioned in my last paper, which more properly belongs to another course of essays, and would be less interesting in a detached form.

If $x = A - B + C - D + E - F +$ &c.

$$\text{Then is } x = \cfrac{A}{1 + \cfrac{B}{A-B + \cfrac{AC}{B-C + \cfrac{BD}{C-D + \cfrac{CE}{D-E + \&c.}}}}}$$

By means of this theorem, a multitude of series may be turned into forms, distinguished for elegance, and convenience to the memory, as any one may convince himself by trial with the common circular, logarithmic and other series. The great drawback on the score of utility is this, that the fraction and the series it represents, proceed *pari passu;* the aggregate of any number of terms of the one, being the same as that of an equal number of terms of the other. So that the fraction is merely a transformation of the series, and not an expression for its sum.

To give a single instance: let this be the hypergeometric series $1 - 2 + 6 - 24 + 120 -$ &c. Turned into a continued fraction by the above formula, it becomes

$$\cfrac{1}{1 + \cfrac{2}{-1 + \cfrac{6}{-4 + \cfrac{48}{-18 + \&c.}}}}$$

Or, every where *dividing two successive numerators and the denominator belonging to the first of them, by a common measure of the three,** this formula becomes

$$\cfrac{1}{1 + \cfrac{2}{-1 + \cfrac{3}{-2 + \cfrac{4}{-3 + \&c.}}}}$$

the law of continuation being manifest.

Here 1 is become $\frac{1}{1}$, $1-2$ is become $\cfrac{1}{1 + \cfrac{2}{-1}}$, and so forward.

But as regards the value of the series, we only discover that the successive links of the continued fraction converge very evidently toward the limit $\frac{1}{-1}$, which, abstracting the sign, is always too small; that consequently the aggregate of m successive links is still more nearly $= \frac{1}{-1}$, which abstracting the sign is too small or too great, as m is odd or even; and that consequently the whole series has for its limits $\frac{1}{0}$ and $\frac{-1}{0}$; a conclusion to which the series itself would have conducted us equally well.

2. In Prob. 51 of the *Med. Alg.* Waring has remarked that

* If this simple mode of reduction had occurred to Euler, he would not, I think, have said of his denominators b, c, d, &c. in § 368, "arbitrio nostro relinquuntur," and have thus left his readers to conclude, that the values adopted by him were preferred only on account of their convenience—"ita autem eos assumi convenit"—and that a different solution from his might be obtained by making a different choice of denominators; a conclusion which would be quite erroneous.

the arithmetical process for reducing an infinite decimal to continued fractions, and thence to a rational fraction, when possible, may be applied to literal quantities also; which he exemplifies by reducing two easy recurring series. The general bearing and chief use of the principle do not seem to have occurred to him, and hence his remark is of little value.

In the Memoirs of the *Académie des Sciences*, 1772, I understand Lagrange has proposed a method not very different from this, for discovering from the known sum of a series whether the latter is recurrent or not; viz. by reducing the reciprocal of the sum to continued fractions, extending each quotient as far as *two terms.* Every object proposed by these distinguished writers appears to be *included* in the following simple and general train of deduction.

The most convenient expression for a series whose sum is to be found or approximated by continued fractions is,

$$\varphi = a - a\,a_1 + a\,a_1\,a_2 - a\,a_1\,a_2\,a_3 + \&c. \ldots\ldots \quad (1)$$

which is perfectly general, while it favours the fractional reductions that follow. Put $P = p - a_1$, $Q = q - a_1 p + a_1 a_2$, $R = r - a_1 q + a_1 a_2 p - a_1 a_2 a_3$, &c. p, q, r, &c. being indeterminate, and we have

$$\varphi = \frac{1 + P + Q + R + \ldots}{1 + p + q + r + \ldots} \cdot a \ldots . (2)$$

Make this $= \frac{a}{1 + \varphi_1}$; then is

$$\varphi_1 = \frac{1 + (p - a_2) + (q - a_2 p + a_2 a_3) + \ldots}{1 + (p - a_1) + (q - a_1 p + a_1 a_2) + \ldots} \cdot a_1$$

Make this $= \frac{a_1}{1 + \varphi_2}$, and let $\beta_1 = a_2 - a_1$, and generally $\beta_u = a_{u+1} - a_1$; then

$$\varphi_2 = \frac{\beta_1 + (\beta_1 p - \beta_2 a_2) + (\beta_1 q - \beta_2 a_2 p + \beta_3 a_2 a_3) + \ldots}{1 + (p - a_2) + (q - a^2 p + a_2 a_3) + \ldots}$$

Make this $= \frac{\beta_1}{1 + \varphi_3}$, and put $\gamma_u = \beta_{u+1} - \beta_1 = a_{u+2} - a_2$; then is

$$\varphi_3 = \frac{\gamma_1 + (\gamma_1 p - \gamma_2 a_3) + (\gamma_1 q - \gamma_2 a_3 p + \gamma_3 a_3 a_4) + \ldots}{\beta_1 + (\beta_1 p - \beta_2 a_2) + (\beta_1 q_1 - \beta_2 a_2 p + \beta_3 a_2 a_3) + \ldots} \cdot a_2$$

Make this $= \frac{a_2 \gamma_1}{\beta_1 + \varphi_4}$, and put $\delta_u = a_{u+2}\,\beta_1\,\gamma_{u+1} - a_2\,\beta_{u+1}\,\gamma_1$; then is

$$\varphi_4 = \frac{\delta_1 + (\delta_1 p - \delta_2 a_3) + (\delta_1 q - \delta_2 a_3 p + \delta_3 a_3 a_4) + \ldots}{\gamma_1 + (\gamma_1 p - \gamma_2 a_3) + (\gamma_1 q - \gamma_2 a_3 p + \gamma_3 a_3 a_4) + \ldots}$$

Make this $= \frac{\delta_1}{\gamma_1 + \varphi_5}$; and $\varepsilon_u = \gamma_1\,\delta_{u+1} - \gamma_{u+1}\,\delta_1$; then is

$$\varphi_5 = \frac{\varepsilon_1 + (\varepsilon_1 p - \varepsilon_2 a_4) + (\varepsilon_1 q - \varepsilon_2 a_4 p + \varepsilon_3 a_4 a_5) + \ldots}{\delta_1 + (\delta_1 p - \delta_2 a_3) + (\delta_1 q - \delta_2 a_3 p + \delta_3 a_3 a_4) + \ldots} \cdot a_3$$

Since the expression for φ_5 is of the same form as that for φ_3, we now possess the law for continuing the series, the rest of

which will, therefore, consist of a continual alternation of terms similar to the last two. The collected result is,

$$\phi = \frac{a}{1} + \frac{a_1}{1 + \frac{a_2\,\gamma_1}{\beta_1 + \frac{\delta_1}{\gamma_1 + \frac{a_3\,\varepsilon_1}{\delta_1 + \frac{\zeta_1}{\varepsilon_1 + \&c.}}}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

The continued fraction is exactly the same as would have resulted from performing the like operation on the series itself; but there is a manifest advantage in having all the elements of convergency in one view.

3. When any of the quantities ϕ becomes $= 0$, the fraction is terminated, and the series is truly summed. E. g. If $\phi_5 = 0 \therefore \varepsilon_1 = 0$, and not ε_1 only, but ε_2, ε_3, &c. since p, a_4, a_5, &c. are to be left perfectly arbitrary. Whence $\phi_4 = \frac{\delta_1}{\gamma_1}$, the other terms all vanishing, and that individually, viz. $\gamma_1\, p - \gamma_2\, a_3 = 0$, $\delta_1\, p - \delta_2\, a_3 = 0$, &c. on the same principle. Therefore $\phi_3 = \frac{a^2\,\gamma_1}{\beta_1 + \frac{\delta_1}{\gamma_1}}$ is to be identical with $\frac{a_2\,\gamma_1}{\beta_1 + (\beta_1\, p - \beta_2\, a_2)}$, the remaining terms vanishing, and separately as before. In the same manner $\phi_2 = \frac{\beta_1 + \beta_1\, p - \beta_2\, a_2}{1 + (p - a_2)}$, $\phi_1 = \frac{1 - (p - a_2)}{1 + (p - a_1) + (q - a_1\, p + a_1\, a_2)}$, $\phi = \frac{1 + P + Q}{1 + p + q}$. The remaining quantities R, S, &c. r, s, &c. are therefore each $= 0$, and their equations reduce to

$$\begin{aligned} q - a_2\, p + a_2\, a_3 &= 0 \\ q - a_3\, p + a_3\, a_4 &= 0 \\ q - a_4\, p + a_4\, a_5 &= 0 \text{ \&c. } \ldots\ldots\ldots\ldots \quad (4) \end{aligned}$$

Conversely, if the series be recurring, the fraction will terminate, as will appear by reversing this reasoning, since the sum of every such series is expressible by a finite portion of $\frac{1 + P + Q + \ldots\ldots}{1 + p + q + \ldots\ldots}\, a$.

If the series do not terminate, the continued fraction nevertheless affords an approximation towards the value of the series, and means of estimating the degree of approximation, viz. either from the law of continuation of the fraction itself, or by the equations for P, Q, R, &c.

While the quantities ϕ are all affirmative, the fraction is certainly convergent toward the correct value of the series.

By the aid then of the successive formulæ in Art. 2, any series, but particularly such as are distinguished by alternating signs, may be summed or algebraically approximated by continued fractions, and in general arithmetically too, *without* the tedious process of *direct division*.

4. In illustration of these general remarks, let it be proposed

to find, or approximate toward, the sum of any series whose successive terms are the continual product of quantities in arithmetical progression. The general value of a_u being $\frac{m + (u-1)r}{n + (u-1)s}$, we have

$$\beta_u = \frac{m+ur}{n+us} - \frac{m}{n} = \frac{u(nr-ms)}{n(n+us)} = \frac{u(n+s)}{n+us}\beta_1$$

$$\gamma_u = \beta_{u+1} - \beta_1 = \frac{un}{n+(u+1)s}\cdot\beta_1 = \frac{u(n+2s)}{n+(u+1)s}\gamma_1$$

$$\delta_u = \frac{m+(u+1)r}{n+(u+1)s}\beta_1\gamma_{u+1} - \frac{m+r}{n+s}\beta_{u+1}\gamma_1 = \frac{u\,.\,u+1\,.\,nr-ms+rs}{n+(u+1)s\,.\,n+(u+2)s}$$

$$\times\beta_1\gamma_1 = \frac{u\,.\,n+1\,.\,n+2s\,.\,n+3s}{1\,.\,2\,.\,n+(u+1)s\,.\,n+(u+2)s}\delta^1$$

and so on. The final reduction at each step is preparatory to the next step. Continuing the process till ε_u, ζ_u, η_u, are found, the law of continuation becomes certain, and then putting $y = 1$, we have

$$\beta_1 = \frac{nr-ms}{n\,.\,n+s} \cdots\cdots \quad \gamma_1 = \frac{n\,.\,n+s}{n+s\,.\,n+2s}\beta_1 \quad \therefore a_2\gamma_1 = \frac{m+r\,.\,n}{n+s\,.\,n+2s}\cdot\beta_1$$

$$\delta_1 = \frac{2(nr-ms+rs)}{n+2s\,.\,n+3s}\beta_1\gamma_1 \quad \varepsilon_1 = \frac{n+s\,.\,n+2s}{n+3s\,.\,n+4s}\gamma_1\delta_1 \quad \therefore a_3\varepsilon_1 = \frac{m+2r\,.\,n+s}{n+3s\,.\,n+4s}\gamma_1\delta_1$$

$$\eta_1 = \frac{3(nr-ms+2rs)}{n+4s\,,\,n+5s}\delta_1\varepsilon_1 \quad \eta_1 = \frac{n+2s\,.\,n+3s}{n+5s\,.\,n+6s}\varepsilon_1\zeta_1 \quad \therefore a_4\eta_1 = \frac{m+3r\,.\,n+2s}{n+5s\,.\,n+6s}\varepsilon_1\zeta_1$$

&c. &c.

When these quantities are introduced in their places in Eq. 3, and successive reductions applied on the principle already intimated, viz. that *any two successive numerators, and the denominator belonging to the first of them, may be multiplied or divided by any common factor, without altering the value of the series,* the value of

$$\phi = a - \frac{m}{n}\cdot a + \frac{m\,.\,m+r}{n\,.\,n+s}a - \frac{m\,.\,m+r\,.\,m+2r}{n\,.\,n+s\,.\,n+2s}\cdot a + \text{\&c.} \quad \ldots (5)$$

becomes in its most disencumbered form.

$$\phi = \cfrac{a}{1 + \cfrac{m}{n + \cfrac{nr-ms}{n+s + \cfrac{(m+r)n}{n+2s + \cfrac{2(nr-ms+rs)}{n+3s + \cfrac{(m+2r)(n+s)}{n+4s + \cfrac{3(nr-ms+2rs)}{n+5s + \text{\&}}}}}}}} \quad \ldots (6)$$

the law of continuation being manifest.

This formula (5) includes a large class of useful series, of which the following are a specimen:

Example I.—Tan.$^{-1}\,t$, or arc. tan. t =

$$t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \text{\&c.} =$$

$$t - \frac{t^2}{3}\cdot t + \frac{t^2\,.\,3t^2\,.\,5t^2}{3\,.\,5\,.\,7}\,.\,t + \text{\&c. becomes}$$

$$\tan.^{-1} t = \frac{t}{1+}\,\frac{t^2}{3+}\,\frac{2^2 t^2}{5+}\,\frac{3^2 t^2}{7+}\,\frac{4^2 t^3}{9} + \&c.$$

Example II.—Log. $(a + 1) = a - \frac{1}{2} a^2 + \frac{1}{3} a^3 - \frac{1}{4} a^4 +$ &c. or $a - \frac{a}{2} \cdot a + \frac{a \cdot 2a}{2 \cdot 3} \cdot a - \frac{a \cdot 2a \cdot 3a}{2 \cdot 3 \cdot 4} \cdot a +$ &c. becomes

$$\frac{a}{1+}\,\frac{1^2 a}{2+}\,\frac{1^2 a}{3+}\,\frac{2^2 a}{4+}\,\frac{2^2 a}{5+}\,\frac{3^2 a}{6+}\,\frac{3^2 a}{7} + \&c.$$

Example III.—Given $\varepsilon^{-x} = 1 - \frac{x}{1} + \frac{x^2}{2} - \frac{x^3}{2 \cdot 3} + \frac{x^4}{2 \cdot 3 \cdot 4} -$ &c. or $1 - \frac{x}{1} + \frac{x \cdot x}{1 \cdot 2} - \frac{x \cdot x \cdot x}{1 \cdot 2 \cdot 3} +$ &c. This, therefore, becomes

$$\frac{1}{1+}\,\frac{x}{1-}\,\frac{1x}{2+}\,\frac{1x}{3-}\,\frac{2x}{4+}\,\frac{2x}{5-}\,\frac{3x}{6+}\,\frac{3x}{7} - \&c.$$

$$= \frac{1}{1+}\,\frac{x}{1-}\,\frac{x}{2+}\,\frac{x}{3-}\,\frac{x}{2+}\,\frac{x}{5-}\,\frac{x}{2+}\,\frac{x}{7} - \&c.$$

Example IV.—Segment to versed sines v, V, or

$$\frac{2v\sqrt{Vv}}{3}\left(1 + \frac{v}{5V} - \frac{v}{7V} \cdot \frac{v}{5V} + \frac{v \cdot 3v}{7V \cdot 9V} \cdot \frac{v}{5V} - \frac{v \cdot 3v \cdot 5v}{7V \cdot 9V \cdot 11V} \cdot \frac{v}{5V} + \&c.\right)$$

$$= \frac{2v\sqrt{Vv}}{3}\left(1 + \frac{v}{5V+}\,\frac{1 \cdot 5v}{7+}\,\frac{2 \cdot 6v}{9V+}\,\frac{3 \cdot 7v}{11V+}\,\frac{4 \cdot 8v}{13V} + \&c.\right)$$

All these formulæ converge much more rapidly than the series they represent, and afford a ready mode of approximating their values, when the arbitraries t, a, x, v, &c. are given in small integers or fractions.

(*To be continued.*)

Article IV.

Astronomical Observations, 1826.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

May 1. Emersion of Jupiter's first satellite	8h	49′	50″	Mean Time at Bushey.
	8	51	11	Mean Time at Greenwich.
May 1. Emersion of Jupiter's second satellite	10	30	29	Mean Time at Bushey.
	10	31	50·7	Mean Time at Greenwich.

April 16. Occultation κ Cancer by the Moon.

Immersion	12	57	48·5	Sidereal Time.
Emersion	13	15	02	

N. B. Immersion instantaneous, and no perceptible diminution of the star's light. At the Emersion the moon's limb was tremulous.

Article V.

On the Alterations that must necessarily be made in the System of Chemical Mineralogy, in consequence of the Property of Isomorphous Bodies to replace one another in indefinite Proportions. By M. J. Berzelius.

(*Concluded from* p. 386.)

Classification of Minerals according to their most Electro-negative Element.

Class I.—*Minerals composed after the Manner of Inorganic Substances.*

1st *Family. Iron.*
Meteoric iron .... Fe (Ni Co Ch)

2\. *F. Copper.*
Native copper .... Cu

3\. *F. Bismuth.*
Native bismuth .... Bi

4\. *F. Silver.*
Native silver .... Ag

5\. *F. Mercury.*
Native mercury .... Hg
Amalgam .... Ag Hg^2

6\. *F. Palladium.*
Native palladium .... Pa

7\. *Platina.*
Native Platina .... Pt

8\. *F. Osmium.*
Osmiuret of iridium .... Ir Os^x

9. *F. Gold.*	
Native gold	Au
Electrum, or argentiferous gold	$Ag\ Au$
10. *F. Tellurium.*	
Native tellurium	Te
Tellurets.	
Telluret of bismuth	$Bi\ Te_x$
——— lead	$(Au\ Te^3 + 4\ Pb\ Te^2 (+ 2\ Pb\ S^2)$
——— silver (*foliated tellurium*)	$Ag\ Te^2 + 2\ Pb\ Te^2 + 3\ Au\ Te^3$
Graphic tellurium	$Ag\ Te^2 + 3\ Au\ Te^3$
11. *F. Antimony.*	
Native antimony	Sb
Stibiurets.	
Antimonial silver	$Ag^2\ Sb$
12. *F. Arsenic.*	
Native arsenic	As
Arseniurets.	
Arsenical nickel. (kupfernickel)	$Ni\ As$
———	$Ni\ As^2$
Arsenical cobalt	$Co\ As$
———	$Co\ As^2$
Arseniated bismuth	$Bi\ As^x$
——— copper	$Cu\ As^x$
——— silver	$Ag\ As^x$
——— antimony. (*testaceous*)	$Sb\ As^x$
13. *F. Carbon.*	$C.$
Diamond	
Fossil coal.	
Anthracite.	
Carburets.	
Graphite	$F\ C_x$
14. *F. Azote.*	
Nitrogen gas	$Az.$
15. *F. Selenium.*	
Seleniurets.	
Seleniated lead*	$Pb\ Se^2$
Seleniated copper	$Cu\ Se$
Eukairite	$2\ Cu\ Se + Ag\ Se^2$
16. *F. Sulphur.*	
Native sulphur	S
Sulphurets.—Sulphuret of manganese	$Mn\ S^2$
Sulphuret of zinc (blende)	$Zn\ S^2$
Pyrites.	
a. Yellow pyrites, *b.* White pyrites	$Fe\ S^4$
c. Magnetic pyrites	$Fe\ S^4 + 6\ Fe\ S^2$
Sulphuret of cobalt	$Fe\ S^4 + 4\ Cu\ S + 12\ Co\ S^3$
Sulphuret of nickel	$Ni\ S^2$
Sulphuret of copper.	
a. Grey sulphuret of copper	$Cu\ S$
b. Hepatic sulphuret of copper	$Fe\ S^2 + 4\ Cu\ S$
c. Pyritous sulphuret of copper	$Cu\ S + Fe\ S^3$
Galena	$Pb\ S^2$
Sulphuret of bismuth	$Bi\ S^2$
Nadelerz	$Pb\ S^2 + 2\ Cu\ S + 2\ Bi\ S^2$

* From the Hartz. Analysed by M. H. Rose, who has lately discovered several mineralized seleniuretted metals.

Cupriferous sulphuret of bismuth	$2\,Bi\,S^2 + 3\,Cu\,S$?
Tin pyrites	$Sn\,S^2 + 2\,Cu\,S$
Sulphuret of silver	$Ag\,S^2$
Argentiferous sulphuret of copper	$2\,Cu\,S + Ag\,S^2$
Wismuth-bleyerz	$Fe\,S^2 + Ag\,S^2 + 2\,Pb\,S^2 + 2\,Bi\,S^2$?
Cinnabar	$Hg\,S^2$
Sulphuret of antimony	$Sb\,S^3$
Nickel-spiesglanzerz	$Ni\,As$, $Ni\,Sb$, $Sb\,S^3$
Bournonite	$Cu\,S + Pb\,S^2 + Sb\,S^3$
Endellione ? (*Schwartzerz*)	$Cu\,S + x\,Sb\,S^3$
Weisgultigerz.	
a. Dunkel	$Pb\,S^2$, $Sb\,S^3$
b. Licht	$Pb\,S^2$, $Ag\,S^2$, $Sb\,S^3$, $Ni\,As$
Grey copper.	
Red silver	$2\,Sb\,S^3 + 3\,Ag\,S^2$
Sulphuret of molybdena	$Mo\,S^3$
Sulphuret of arsenic.	
a. Realgar	$As\,S^2$
b. Orpiment	$As\,S^3$
Sulpho-arseniurets.	
Mispickel	$Fe\,S^4 + Fe\,AS^2$
Grey cobalt	$Co\,S^4 + Co\,AS^2$
Grey nickel	$Ni\,S^4 + Ni\,AS^2$

17. *Oxygen.*

Oxygen gas	O
a. Electro-positive oxides.	
Deutoxide of manganese	$\dddot{Mn}$ *Mn*
Metalloidal manganese	$\ddddot{Mn}$
Red zinc	$\ddot{Zn}$ *Zn*
Oligiste iron	$\ddot{Fe}$ *F*
Oxydulous iron	$\ddot{Fe}\,\dddot{Fe}^2\ f\,F^3$
Franklinite	$\ddot{Zn}\,\dddot{Fe}^2 + \ddot{Mn}\,\dddot{Fe}^2\ \left\{\begin{matrix} nZ \\ mn \end{matrix}\right\} F^3$
Earthy cobalt	$\dddot{Co} + \dddot{Mn} + 3\,Aq$
Red copper	$\dot{Cu}$
Black copper	$\ddot{Cu}$
Massicot	$\ddot{Pb}$
Minium	$\dddot{Pb}$
Oxide of bismuth	$\ddot{Bi}$
Black uranium	$\ddot{U}$
Oxide of tin	$\ddddot{Sn}$
b. Electro-negative oxides.	
Water	$\dot{H}$ *Aq*
Hydrates.	
Brucite	$\ddot{Mg}\,Aq^2$ *M Aq*

Dull manganese	$\dddot{Mn}\ Aq$, $Mn^3\ Aq$
Hydrated oxide of iron	$\ddot{Fe}^2\ Aq^3$, $Fe^2\ Aq$
Hydrated oxide of uranium	$\dddot{U}\ Aq^x$
Alumina.	
Corundum	$\dddot{Al}$, A
Aluminates.	
Spinelle	$M\ A^6$
Pleonaste	$\left\{ \begin{matrix} M \\ f \end{matrix} \right\} A^6$
Gahnite	$Zn\ A^6$
Candite	$M\ A^2 + F\ A^2$
Plomb-gomme	$Pb\ A^6 + 6\ Aq$
Gibbsite	$A\ Aq$
———	$F^2\ Aq + 3\ A^2\ Aq$
Diaspore	$\left\{ \begin{matrix} A^3 \\ F^3 \end{matrix} \right\} Aq$
Silica, with its varieties of crystalline forms, aggregation and colours.	
a. Silicates with a single base.	
1. *Silicates of lime.*	
———	$C\ S^3$
Wollastonite* (tabular spar)	$C\ S^2$
2. *Silicates of magnesia.*	
Serpentine	$M\ S^3$
Steatite	$M\ S^3 + \frac{1}{4}\ Aq$
Ecume de mer	$M\ S^3 + 2\ Aq$
Pyrallolite	$M\ S^2$
Marmalite	$M\ S + Aq$
Hydrosilicates of magnesia.	
Noble serpentine	$M\ S^3 + M\ Aq$
Serpentine from Gullsjo ?†	$M\ Aq^2 + 2\ M\ S^2$
3. *Silicate of zinc*	$Zn\ S + \frac{1}{2}\ Aq$
4. *Silicates of manganese.*	
——— red	$Mn\ S^2$
——— black	$Mn\ S + Aq$
——— metalloidal?	$Mn^3\ S$
5. Silicate of cerium (cerite)	$Ce\ S$
6. *Silicates of iron.*	
Hisingerite.	
Chlorophæite.	
Chloropal	$f\ S^3 + 3\ Aq$
7. *Silicates of copper.*	
Dioptase	$Cu\ S^2 + 2\ Aq$?
Siliciferous malachite.	
8. Silicate of zircon (zircon, hyacinth)	$Zr\ S$

* This substance so coincides in the directions of its cleavages and measurements by the reflective goniometer with *tabular spar*, that it can scarcely be said to be even a variety of that substance; and we fully agree with Mr. Phillips in hoping, "that the name given to it by Leman, in honour of Dr. Wollaston, will be abandoned, and that we shall ere long find the designation of Wollastonite, in honour of one who has done so much for almost every department of science, appropriated to some mineral of a less questionable nature." (Introduction to Mineralogy, 3d Edit. p. 211.)—*Ed.*

† Colourless, translucid. Analysed by M. Mosander.

9. *Silicates of alumina.*

Disthene ... $A^2 S$
Plastic clay ... $A S^3$
Blue clay ... $A S^2$?
Clays in general.

b. Silicates with several bases.

1. *Silicates with an alkaline base, with silicate of alumina and water of crystallization.*

Zeolites
Apophyllite ... $K S^6 + 8 C S^3 + 16 Aq$
Chabasie
a. —— with base of soda ... $\left.\begin{matrix}N\\K\end{matrix}\right\} S^2 + 3 A S^2 + 6 Aq$
b. —— with base of lime (Levyne) ... $\left.\begin{matrix}C\\N\\K\end{matrix}\right\} S^2 + 3 A S^2 + 6 Aq$
Mesotype ... $N S^3 + 3 A S + 2 Aq$
Mesolite ... $N S^3 + 2 C S^3 + 9 A S + 8 Aq$
—— from Hauenstein ... $N S^3 + C S^3 + 6 A S + 6 Aq$
Mesola ... $N S^2 + 2 C S^3 + 9 A S + 8 Aq$
Analcime ... $N S^2 + 3 A S^2 + 2 Aq$
Thomsonite ... $N S^2 + 3 C S + 12 A S + 10 Aq$
Stilbite ... $C S^3 + 3 A S^3 + 6 Aq$
—— dodecahedral, lamelliform ... $\left.\begin{matrix}C\\N\end{matrix}\right\} S^3 + 3 A S^3 + 6 Aq$
Heulandite ... $C S^3 + 4 A S^3 + 6 Aq$
Brewsterite ... $\left.\begin{matrix}C\\N\end{matrix}\right\} S^3 + 4 A S^3 + 8 Aq$
Laumonite ... $C S^2 + 4 A S^2 + 6 Aq$
Scolezite ... $C S^3 + 3 A S + 3 Aq$
Harmotome ... $B S^4 + 4 A S + 6 Aq$
Prehnite ... $C^2 S^3 + 3 A S + Aq$

2. *Anhydrous silicates, with an alkaline base and silicate of alumina.*

Felspar ... $K S^3 + 3 A S^3$
Albite ... $N S^3 + 3 A S^3$
Petalite ... $L S^6 + 3 A S_3$
Triphane ... $L S^3 + 3 A S^2$
Spodumene with base of soda ... $\left.\begin{matrix}N\\K\\C\\M\end{matrix}\right\} S^3 + 3 A S^2$
Lencite (amphigène) ... $K S^2 + 3 A S^2$
Labrador ... $N S^3 + 3 C S^3 + 12 A S$
Paranthine ... $\left.\begin{matrix}C\\N\end{matrix}\right\} S^2 + 2 A S$
Meionite.
Scapolite.
Wernerite.
Ekebergite ... $C S^2 + 3 N S^2 + 8 A S$
Elæolite ... $\left.\begin{matrix}N\\K\end{matrix}\right\} S + 3 A S$
Nepheline ... $N S + 3 A S$
Sodalite ... $N S^2 + 2 A S$
Ittnerite ... $C S + 2 N S + 9 A S$
Anhydrous scolezite ... $C S^3 + 3 A S$
Andalusite ?

Appendix.

Perlstein, sphærulite.
Resinite.
Obsidian.
Marekanite.

3. *Silicates with an alkaline base; with silicates of magnesia (occasionally replaced by the oxidules of iron or manganese) and silicate of alumina.*

Talc.

Agalmatolite.

Pimelite.

Cimolite.

Chlorite.

Green earth.

Micas.

a. — with base of potassa.

b. — magnesian.

c. — with base of potassa and lithia.

Gieseckite.

Pinite.

Fahlunite.

4. *Silicate with an alkaline base, with silicate of iron.*

Achmite. . . . . $N S^3 + 2 F S^2$

5. *Silicates of lime with silicates of magnesia, often replaced by oxidulous iron, more rarely by oxydulous manganese; and the silica sometimes replaced by alumina.*

Pyroxene.

a. White pyroxene . . . . $C S^2 + M S^2$

b. Green pyroxene . . . . $C S^2 + \left.\begin{matrix}M\\f\end{matrix}\right\} S^2$

c. Hedenbergite . . . . $C S^2 + f S^2$

d. Manganesiferous . . . . $C S^2 \left.\begin{matrix}M\\f\\mn\end{matrix}\right\} S^2$

e. Augite . . . . $C S^2 + \left.\begin{matrix}M\\f\end{matrix}\right\} \left\{\begin{matrix}S^2\\A^2\end{matrix}\right.$

Amphibole.

a. Grammatite . . . . $C S^3 + M S^2$

b. Actinote . . . . $C S^2 + \left.\begin{matrix}M\\f\end{matrix}\right\} S^2$

c. Hornblende . . . . $C S^3 + \left.\begin{matrix}M\\f\end{matrix}\right\} \left\{\begin{matrix}S^2\\A^2\end{matrix}\right.$

6. *Silicates of lime, magnesia, and the oxydules of iron and manganese.*

Ilvaite . . . . $C S + 4 f S$

Cronstedtite . . . . $mn\, S + 6 f S + 9 Aq$

Pyrosmalite . . . . $mn\, S^2 + f S^2$

Chrysolite (olivine) . . . . $\left.\begin{matrix}M\\f\end{matrix}\right\} S$

Diallage . . . . $f S^2 + 3 M S^2$

Hypersthene . . . . $f S^2 + M S^2$

7. *Silicates of lime often replaced by magnesia, or by the oxydules of iron or manganese; with silicates of alumina, occasionally replaced by peroxide of iron.*

Epidote.

a. Zoisite . . . . $C S + 2 A S$

b. Pistacite . . . . $\left.\begin{matrix}C\\f\end{matrix}\right\} S + 2 A S$

Idocrase.

a. Vesuvian (common).

b. Loboite (containing magnesia).

c. Cyprina (cupriferous).

Essonite.

Garnet.

a. Grossular .... $CS + AS$
b. Aplome .... $CS + FS$
c. Almandine .... $fS + AS$
d. Magnesian garnet.
e. Manganesian garnet.
f. Pyrope .... $\left.\begin{matrix}C\\M\\f\\Chr\end{matrix}\right\}S + S$
g. Mixed garnets .... $\left.\begin{matrix}C\\M\\f\\mn\end{matrix}\right\}S + \left.\begin{matrix}A\\F\end{matrix}\right\}S$

Gehlenite .... $2CS + \left.\begin{matrix}A^2\\F^2\end{matrix}\right\}S$
Anthophyllite.
Cerine (allanite) .... $CS + 2AS, ce S, fS$
Dichroite .... $\left.\begin{matrix}M\\f\\mn\end{matrix}\right\}S^2 + 3AS$
Nephrite.
Steatite .... $MS^2 + AS^2$
Sordanvalite .... $MS^2 + 2fS^2 + 3AS^2$
Karpholite .... $\left.\begin{matrix}mn\\f\end{matrix}\right\}S + 3AS + 9Aq$
Siliceo-aluminates: Saphirin .... $\left.\begin{matrix}M\\f\end{matrix}\right\}S + 5A\left.\begin{matrix}M\\f\end{matrix}\right\}\left\{\begin{matrix}A^6\\S^6\end{matrix}\right.$
Chamoisite .... $f^2A + 2fS$

8. *Silicates of oxide of iron and alumina.*
Staurotide .... $\left.\begin{matrix}A^4\\F^4\end{matrix}\right\}S$

9. *Silicates of glucina and alumina.*
Smaragdite (emerald) .... $GS^4 + 2AS^2$
Euclase .... $GS^2 + 2AS$
Cymophane .... ? $A^4S + 2GA^4$

10. *Silicates of yttria with silicates of iron, cerium, &c.*
Gadolinite.
a. —— from Ytterby .... $ce^2S + f^2S + 4YS$
b. —— from Kararfvet .... $\left\{\begin{matrix}YS, CS^2, mn S\\fS, GS, ce S\end{matrix}\right.$
Orthite.
Pyrorthite.

11. *Silicates of zirconia, with other silicates.*
Eudyalite .... $\left.\begin{matrix}N\\f\\Z\\C\\mn\end{matrix}\right\}S$

Oxide of titanium (*titanic acid*) .... $\overset{\cdots}{Ti}$
a. Rutilite.
b. Anatase.

Titanates.
a. Titanates of protoxide of iron.
b. Titanates of oxide of iron.
Crichtonite.
Polymignite.*

* From Fredricwern, in Norway. This mineral is composed of titanic acid combined with zirconia, lime, yttria, and the protoxides of iron, manganese, and cerium. It contains besides, traces of potassa, magnesia, silica and oxide of tin. The name *polymignite* signifies a mixture of many substances.

Siliceo-titanates.

Sphene. .... $C\,Ti^6 + C\,S^6$

Tantalic acid.................... $\dddot{\mathrm{Ta}}$

Tantalates.

Yttrotantalite.

a. —— black .... $\left.\begin{array}{l}\ddot{\mathrm{Ca}}_3\\ \ddot{\mathrm{Y}}_3\\ \ddot{\mathrm{Fe}}^3\end{array}\right\}\begin{array}{l}\dddot{\mathrm{Ta}}^2\\ \\ \dddot{\mathrm{W}}^2\end{array}$

b. —— brown.................... $\left.\begin{array}{l}\ddot{\mathrm{Y}}\\ \ddot{\mathrm{Ca}}^3\end{array}\right\}\dddot{\mathrm{Ta}}$

c. —— yellow.................... $\left.\begin{array}{l}\ddot{\mathrm{Y}}^3\\ \dddot{\mathrm{U}}^2\end{array}\right\}\dddot{\mathrm{Ta}}^2$

Tantalite.

a. —— from Kimito.................... $\ddot{\mathrm{Mn}}\,\dddot{\mathrm{Ta}}^2 + \ddot{\mathrm{Fe}}\,\dddot{\mathrm{Ta}}^2$

b. —— from Finbo.................... $\left.\begin{array}{l}\dddot{\mathrm{Mn}}\\ \ddot{\mathrm{Fe}}\end{array}\right\}\begin{array}{l}\dddot{\mathrm{Ta}}^2\\ \ddddot{\mathrm{Sn}}\end{array}$

c. —— from Brodbo .... $\left.\begin{array}{l}\ddot{\mathrm{Mn}}\\ \ddot{\mathrm{Fe}}\\ \ddot{\mathrm{Ca}}\end{array}\right\}\left\{\begin{array}{l}\dddot{\mathrm{Ta}}^2\\ \dddot{\mathrm{W}}^2\\ \ddddot{\mathrm{Sn}}\end{array}\right.$

d. —— from Bayern.................... $\ddot{\mathrm{M}}^3\,\dddot{\mathrm{Ta}}^4 + 4\,\ddot{\mathrm{Fe}}^3\,\dddot{\mathrm{Ta}}^4$

e. —— from Kimito, but giving a cinnamon-coloured powder .... $\left.\begin{array}{l}\ddot{\mathrm{Fe}}\\ \ddot{\mathrm{Mn}}\end{array}\right\}\dddot{\mathrm{Ta}}^2$

Oxide of antimony .... $\dddot{\mathrm{Sb}}$

Oxide of antimony with sulphuret of antimony (oxi-sulphurette d'antimony).......... $\dddot{\mathrm{Sb}} + 2\,\mathrm{Sb}\,\mathrm{S}^3$

Antimonious acid .... $\ddddot{\mathrm{Sb}}$

Tungstic acid. .... $\dddot{\mathrm{W}}$

Tungstates.

Scheelin.................... $\ddot{\mathrm{Ca}}\,\dddot{\mathrm{W}}^2$

Wolfram. .... $\ddot{\mathrm{Mn}}\,\dddot{\mathrm{W}}^2 + 3\,\ddot{\mathrm{Fe}}\,\dddot{\mathrm{W}}^2$

Tungstate of lead.................... $\ddot{\mathrm{Pb}}\,\dddot{\mathrm{W}}^2$

Molybdic acid .... $\dddot{\mathrm{Mo}}$

Molybdate of lead.................... $\ddot{\mathrm{Pb}}\,\dddot{\mathrm{Mo}}^2$

Oxide of chrome.................... $\dddot{\mathrm{Chr}}$

Chromated iron.

Chromated lead .... $\ddot{\mathrm{Pb}}\,\dddot{\mathrm{Chr}}$

Vauquelinite .... $2\,\ddot{\mathrm{Pb}}^3\,\overset{:::}{\mathrm{Chr}}{}^2 + \ddot{\mathrm{Cu}}^3\,\overset{:::}{\mathrm{Chr}}{}^2$

Boracic acid.

Hydrated boracic acid .... $\dddot{\mathrm{Bo}}\,\mathrm{Aq}^6$

Borates.

Tincal .... $\ddot{\mathrm{Na}}\,\overset{:::}{\mathrm{Bo}}{}^2 + 20\,\mathrm{Aq}$

Boracite .... $\ddot{\mathrm{M}}\,\dddot{\mathrm{Bo}}$

Borosilicates.

Datholite .... $\ddot{\mathrm{Ca}}\,\overset{:::}{\mathrm{Bo}}{}^2 + \ddot{\mathrm{Ca}}\,\dddot{\mathrm{Si}}{}^2 + \mathrm{Aq}$

Botryolite .... $\ddot{\mathrm{Ca}}\,\dddot{\mathrm{Bo}} + \ddot{\mathrm{Ca}}\,\dddot{\mathrm{Si}}{}^2 + \mathrm{Aq}$

Tourmaline.

a. ——— with base of potassa.
b. ——————— lithia.
c. ——————— magnesia.

Axinite.

Carbonic acid .... $\ddot{\mathrm{C}}$

Carbonates.

Carbonate of soda .... $\ddot{\mathrm{Na}}\,\ddot{\mathrm{C}}{}^2$

Witherite .... $\ddot{\mathrm{Ba}}\,\ddot{\mathrm{C}}{}^2$

Strontianite .... $\ddot{\mathrm{Sr}}\,\ddot{\mathrm{C}}{}^2$

Carbonate of lime .... $\ddot{\mathrm{Ca}}\,\ddot{\mathrm{C}}{}^2$

a. Arragonite.

b. Calcareous spar.

Carbonate of magnesia.

a. Hard ....
b. Earthy (*giobertite*) .... $\left.\right\}\ \ddot{\mathrm{M}}\,\ddot{\mathrm{C}}{}^2$

c. —— With water of crystallization .... $\ddot{\mathrm{M}}\,\ddot{\mathrm{C}}{}^2 + 6\,\mathrm{Aq}$

d. White magnesia .... $\ddot{\mathrm{M}}\,\mathrm{Aq}^8 + 3\,\ddot{\mathrm{M}}\,\ddot{\mathrm{C}}{}^2$

e. Dolomite .... $\ddot{\mathrm{Ca}}\,\ddot{\mathrm{C}}{}^2 + \ddot{\mathrm{M}}\,\ddot{\mathrm{C}}{}^2$

f. Miemite.

g. Gurhofianite.

Carbonate of manganese .... $\left.\begin{matrix}\ddot{\mathrm{Ca}}\\ \ddot{\mathrm{Mn}}\end{matrix}\right\}\ddot{\mathrm{C}}{}^2$

Carbonate of iron .... $\ddot{\mathrm{Fe}}\,\ddot{\mathrm{C}}{}^2$

Mixtures of the preceding.

Carbonate of zinc.

a. Calamine .... $\ddot{\mathrm{Zn}}\,\ddot{\mathrm{C}}{}^2$

b. Earthy carb. zinc	$\ddot{Zn}\,Aq^6 + 3\,\ddot{Zn}\,\ddot{C}^2$
Carbonate of cerium	$\ddot{Ce}\,\ddot{C}^2$
Carbonate of lead	$\ddot{Pb}\,\ddot{C}^2$
Carbonate of copper.	
a. Malachite	$\ddot{Cu}\,\ddot{C}^2 + Aq$
b. Blue carb. cop.	$\ddot{Cu}\,Aq^2 + 2\,\ddot{Cu}\,\ddot{C}^2$
c. Siliceous carb. cop.	
Humboldtine (oxalate of iron)	? $\ddot{Fe}\,\overline{O}^3$
Arsenious acid	$\dddot{As}$
Arseniates.	
Pharmacolite	$\ddot{Ca}\,\overset{\cdot\cdot\cdot\cdot\cdot}{As} + 6\,Aq$
Picropharmacolite	$\left.\begin{matrix}\ddot{Ca}^5\\ \ddot{M}^5\end{matrix}\right\}\overset{\cdot\cdot\cdot\cdot\cdot}{As}^4 + 30\,Aq$
Arseniate of iron.	
a. Scorodite.	
b. Cubic arseniate of iron	$\ddot{Fe}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}^2 + 2\,\dddot{Fe}^2\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}^2 + 36\,Aq$
c. Fer resinite.	
Arseniate of cobalt.	
a. Subarseniate.	
b. Subarsenite.	
Arseniate of nickel.	
a. Subarseniate, from Allemont.	$\ddot{Ni}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}^2 + 18\,Aq$
b. Subarseniate	$\ddot{Ni}^2\,\overset{\cdot\cdot\cdot\cdot\cdot}{As} + 18\,Aq$
Arseniate of lead	$\ddot{Pb}\,\overset{\cdot\cdot\cdot\cdot\cdot}{As}$
Arseniate of copper, and its varieties which have been but slightly examined.	
Phosphoric acid.	
Phosphates.	
Apatite	$\ddot{Ca}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}^2$
Wagnerite	$\ddot{M}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}^2$
Phosphate of yttria	$\ddot{Y}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}^2$
Phosphate of iron.	
a. ——— from Cornwall	$\ddot{Fe}^4\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}^3 + 16\,Aq$
b. ——— from Bodenmais	$\ddot{Fe}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}^2 + 12\,Aq$
Phosphate of manganese	$\ddot{Mn}\,\overset{\cdot\cdot\cdot\cdot\cdot}{P} + \ddot{Fe}^2\,\overset{\cdot\cdot\cdot\cdot\cdot}{P}$

Phosphate of lead	$\ddot{Pb}\ \overset{\cdot\cdot\cdot\cdot\cdot}{P}$	
Phosphate of copper.		
a. ———— from Ehrenbreitstein	$\ddot{Cu}^5\ \overset{\cdot\cdot\cdot\cdot\cdot}{P}^2 + 5\ Aq$	
b. ———— from Liebethen	$\ddot{Cu}^2\ \overset{\cdot\cdot\cdot\cdot\cdot}{P} + 2\ Aq$	
Phosphate of alumina.		
a. Wavellite	$\dddot{Al}^4\ \overset{\cdot\cdot\cdot\cdot\cdot}{P}^3 + 12\ Aq$	
b. Lazulite from Kriglach.		
c. Calaite.		
d. Amblygonite	$\ddot{L}^2\ \overset{\cdot\cdot\cdot\cdot\cdot}{P} + \dddot{Al}^4\ \overset{\cdot\cdot\cdot\cdot\cdot}{P}^3$	
e. Phosphate of alumina, with ammonia, from Isle Bourbon.		
Phosphate of uranium.		
a. Uranite	$\ddot{Ca}^3\ \overset{\cdot\cdot\cdot\cdot\cdot}{P} + 4\ \ddot{U}\ \overset{\cdot\cdot\cdot\cdot\cdot}{P} + 48\ Aq$	
b. Chalkolite	$\ddot{Cu}^3\ \overset{\cdot\cdot\cdot\cdot\cdot}{P}^2 + 4\ \ddot{U}\ \overset{\cdot\cdot\cdot\cdot\cdot}{P} + 48\ Aq$	
Fluoric acid.		
Fluates.		
Fluate of lime	$\ddot{Ca}\ \ddot{F}$	
Fluate of yttria	$\ddot{Y}\ \ddot{F}$	
Fluate of cerium.		
a. Neutral	$\dddot{Ce}^2\ \ddot{F}^3$	
b. With excess of base	$\dddot{Ce}^4\ \ddot{F}^3 + 3\ Aq$	
c. Fluate of cerium and yttria	$\left.\begin{matrix}\dddot{Ce}\\ \ddot{Y}\end{matrix}\right\}\ddot{F}$	
d. Yttrocerite	$\left.\begin{matrix}\ddot{Ca}\\ \ddot{Ce}\\ \ddot{Y}\end{matrix}\right\}\ddot{F}$	
Fluate of alumina and soda.		
Chryolite	$3\ \ddot{Na}\ \ddot{F} + \dddot{Al}\ \ddot{F}^3$	
Fluo-silicates.		
Chondrodite.	$\ddot{M}^2\ \ddot{F} + \ddot{M}^3\ \dddot{Si}.$	*M Fl* + 3 *M S*
Pycnite.	$\dddot{Al}^2\ \ddot{F}_3 + 6\ \dddot{Al}\ \dddot{Si}.$	*A Fl* + 3 *A S*
Topaz.	$\dddot{Al}^4\ \ddot{F}^3 + 6\ \dddot{Al}\ \dddot{Si}.$	*A*³ *Fl* + 3 *A S*

Nitric acid.	
Nitrates.	
Saltpetre	$\ddot{\mathrm{K}} + 2\,\ddot{\mathrm{Az}}\,\dddot{\mathrm{Az}}$
Cubic nitre	$\ddot{\mathrm{Na}} + 2\,\ddot{\mathrm{Az}}\,\dddot{\mathrm{Az}}$
Nitrate of lime	$\ddot{\mathrm{Ca}} + 2\,\ddot{\mathrm{Az}}\,\dddot{\mathrm{Az}}$
Nitrate of magnesia	$\ddot{\mathrm{M}} + 2\,\ddot{\mathrm{Az}}\,\dddot{\mathrm{Az}}$
Sulphuric acid.	
a. Liquid	$\dddot{\mathrm{S}}\,\mathrm{Aq}$
b. Sulphurous acid gas	$\ddot{\mathrm{S}}$
Sulphates.	
Sulphate of soda	$\ddot{\mathrm{Na}}\,\dddot{\mathrm{S}}^2 + 20\,\mathrm{Aq}$
Sulphate of baryta	$\ddot{\mathrm{Ba}}\,\dddot{\mathrm{S}}^2$
Celestine	$\ddot{\mathrm{Sr}}\,\dddot{\mathrm{S}}^2$
Gypsum.	
a. Anhydrous	$\ddot{\mathrm{Ca}}\,\dddot{\mathrm{S}}^2$
b. Hydrated	$\ddot{\mathrm{Ca}}\,\dddot{\mathrm{S}}^2 + 4\,\mathrm{Aq}$
c. Glauberite	$\ddot{\mathrm{Na}}\,\dddot{\mathrm{S}}^2 + \ddot{\mathrm{Ca}}\,\dddot{\mathrm{S}}^2$
Sulphate of magnesia	$\ddot{\mathrm{M}}\,\dddot{\mathrm{S}}^2 + 12\,\mathrm{Aq}$
Polyhalite	$\ddot{\mathrm{K}}\,\dddot{\mathrm{S}}^2 + \ddot{\mathrm{M}}\,\dddot{\mathrm{S}}^2 + 2\,\ddot{\mathrm{C}}\,\dddot{\mathrm{S}}^2 + 4\,\mathrm{Aq}$
Sulphate of zinc	$\ddot{\mathrm{Zn}}\,\dddot{\mathrm{S}}^2 + 12\,\mathrm{Aq}$
Sulphate of iron	
a. Green	$\ddot{\mathrm{Fe}}\,\dddot{\mathrm{S}}^2 + 12\,\mathrm{Aq}$
b. Red	$\ddot{\mathrm{Fe}}^3\,\dddot{\mathrm{S}}^4 + 6\,\dddot{\mathrm{Fe}}\,\dddot{\mathrm{S}}^2 + 72\,\mathrm{Aq}$
c. Fibrous.	
d. Ochreous	$\dddot{\mathrm{Fe}}^2\,\dddot{\mathrm{S}} + 6\,\mathrm{Aq}$
Sulphate of cobalt	$\ddot{\mathrm{Co}}_3\,\dddot{\mathrm{S}}^3 + 24\,\mathrm{Aq}$
Sulphate of lead	$\ddot{\mathrm{Pb}}\,\dddot{\mathrm{S}}^2$
a. Cupriferous	$\ddot{\mathrm{Cu}}\,\mathrm{Aq}^2 + \ddot{\mathrm{Pb}}\,\dddot{\mathrm{S}}^2$
b. Sulphato-carbonate	$\ddot{\mathrm{Pb}}\,\ddot{\mathrm{C}}^2 + \ddot{\mathrm{Pb}}\,\dddot{\mathrm{S}}^2$
———— ————	$\ddot{\mathrm{Pb}}\,\ddot{\mathrm{C}}^2 + 3\,\ddot{\mathrm{Pb}}\,\dddot{\mathrm{S}}^2$*

* Or $\ddot{\mathrm{Pb}}\,\dddot{\mathrm{S}}^2 + 3\,\ddot{\mathrm{Pb}}\,\ddot{\mathrm{C}}^2$? viz. *sulphato-tri-carbonate.—Ed.*

Sulphate of alumina.

a. Neutral .... $\dddot{Al}\,\dddot{S}^3 + x\,Aq$

b. With excess of base .... $\dddot{Al}\,\dddot{S} + 9\,Aq$

c. Aluminite.

d. Potassa-alum........................ $\ddot{K}\,\dddot{S}^2 + 2\,\dddot{Al}\,\dddot{S}^3 + 48\,Aq$

e. Ammonia-alum .... $Az^2\,H^6\,\dddot{S} + \dddot{Al}\,\dddot{S}^3 + 26\,Aq$

Sulphate of uranium .... $\ddot{U}\,\dddot{S}^2 + x\,Aq$

Sulphates of uranium and copper.

Appendix: Silicates containing sulphates.

Lapiz lazuli.

Haüyene.

Nosiane.

18. *F. Chlorine.*

Chlorides (muriates).

Sea salt .... $Na\,Ch^4$

Chloride of ammonium .... $Az\,H^4\,Ch^4$

——— calcium .... $Ca\,Ch^4$

——— magnesium........................ $M\,Ch^4$

——— lead...............................

a. —— from Mendip.................. $Pb\,Ch^4 + 2\,\ddot{Pb}$

b. Murio-carbonate of lead, from Matlock $Pb\,Ch^4 + \ddot{Pb}\,\ddot{C}^4$

Chloride of copper .... $Cu\,Ch^4 + 3\,\ddot{Cu} + 8\,Aq$

——— silver............................. $Ag\,Ch^4$

——— mercury .... $Hg\,Ch^4$

Appendix containing chloriferous silicates already mentioned, but which, perhaps, may be better placed near the chlorides.

Sodalite.

Pyrosmalite.

Eudyalite.

Class II.—*Minerals composed after the Manner of Organic Substances, from which they appear to derive their Origin.*

a. Organic substances but slightly changed.

Mould (Humus).
Peat.
Lignite.
Dysodil.

b. Fossil resins.

Yellow amber.
Retin asphaltum.
Elastic bitumen.

c. Fossil oils.

Naphtha.
Petroleum.

d. Bitumens.

Bitumen.
Asphaltum.

e. Coal.

All its varieties.

f. Salts.

Mellite.
Oxalate of iron.

Article VI.

An Account of the Strata North of the Humber, near Cave. By the Rev. William Vernon, PYPS. FRS. FGS.* (With a Plate.) Communicated by the Author.

At the first institution of this Society, it was suggested as a geological subject particularly deserving the attention of its members, to investigate the strata on the eastern side of Yorkshire from the lias to the chalk, and to examine their correspondence with the beds of the great chain which runs northward through England to the Humber.

In this inquiry considerable progress has been made by the late researches of Mr. Smith, who, having at the Society's invitation returned to the county, and resumed his examination of its geology, has explored the northern range of oolitic hills from Scarbro' to Hambleton, with his nephew Mr. Phillips, has detected three members of the southern series which had not been before observed, and established the following descending order of formations; namely, the coral rag, the calcareous grit, the Oxford clay, the Kelloways rock, and the Cornbrash, between which and the lias of Whitby lie several calcareous and sandy beds, occupying the position of the inferior oolite of the south.

It remained an important desideratum to examine with accuracy that part of the oolitic range which was first noticed by Mr. Smith in the neighbourhood of Cave, and which might be expected to furnish the connecting link between the district already mentioned, and the main chain of oolitic hills which traverses the kingdom from beyond Bath, through Lincolnshire to the Humber.

With this view I proposed to the Keeper† of the Museum, to accompany me on an excursion, in which we made the observations now submitted to the Society.

The tract to be investigated was the country to the right and left of the road from Goodmanham to Brough. We knew that at Goodmanham the lias had been found by Mr. Smith, and that he considered it as extending southward to the Humber, and I had myself in 1823 traced the oolite from Sancton nearly to Brough. I had also observed a different calcareous and sandy rock further to the east, and immediately under the chalk an exhibition of red chalk and blue clay similar to that which occurs on the northern side of the Wolds at Specton and at Knapton. The direction then of the lias, the character and extent of two beds belonging to the oolitic series, and of the

* Read before the Yorkshire Philosophical Society, April 4, 1826.

† Mr. Phillips.

above-mentioned clay, were the leading objects of our research.

The most westerly range of rising ground shown by the map (Plate XXXVIII) in this tract is at North and South Cliff, and here, if any where, we conceived the lias was to be traced. Our route was, therefore, directed across the strata from Holme on Spalding Moor, by N. Cliff to Sancton. At Holme we found the summit of the hill to consist of gravel, but the red marle appeared about a mile to the SE with gypsum imbedded in it; the gypsum is worked for plaster, and being divided into two qualities, the one white and pure, the other coloured with grey marle, is sold at the rate of a guinea and twelve shillings a ton The red marle shows itself for some distance on the road from Selby to Cliff, and is then covered by a sandy alluvium which overspreads the country as far as the Cliff hills.

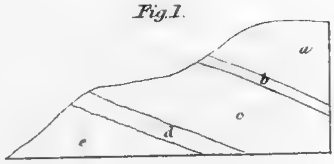
On arriving at North Cliff, we found at once a little to the northward of the village, the first object of our search, and at an opening made for the purpose of burning bricks, discovered the lias well characterised, and containing the distinctive fossils,* of which specimens are before the Society. Mr. Phillips made a sketch of the section which it here presents, the only one to be seen along the whole range. We subsequently traced the course of this stratum southward, with some difficulty from the circumstance of the stone not being worked; it appeared, however, at the surface near South Cliff, and we detected it again at Hotham, at Everthorpe, and at the western end of the village of South Cave; at these places we found its peculiar pentacrinites and gryphites in heaps of blue clay and septaria taken out of wells, one of which had been sunk twelve years ago, but the materials had remained undisturbed. The stone extracted from the well at Everthorpe had a peculiar hollow oolitic structure. We are disposed to assign also to the lias the clay of which bricks are made at Brough, and again on the road from Sancton to Market Weighton. On a hill near the latter place, we found it marked by gryphites in a brick yard; and at a point near Goodmanham, on the road to South Dalton, where I had formerly noticed the red chalk marle, we found under the red chalk a rock distinguished by the same hollow oolitic grains with which we had been struck at Everthorpe, certainly belonging to the same stratum. Beyond this point its outcrop has been observed by Mr. Smith all along the western side of the Wolds by Pocklington and Seppington to Craike, and the lias may, therefore, be considered as having now been traced in an uninterrupted course from the Humber to Whitby.

Our examination of the next range of hills to the eastward commenced at Sancton, where the oolite first appears passing

* Plagiostoma gigantea, Ammonites,
Plagiostoma rusticum, Pentacrinus caput Medusæ.

GEOLOGY OF CAVE &c.

Fig. 1.



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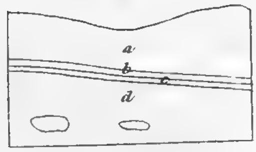

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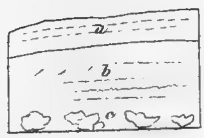
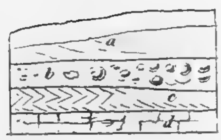


4



5

Chalk

Kimmeridge Clay

Kelloways Rock

Oolite

Lias

J. Shury, sc.

Engraved for the Annals of Philosophy, for Baldwin, Cradock & Joy, June 1 1826.

out from under the chalk, and we pursued it along a continuous line of quarries on the right of the road to Brough, by Ellerker, almost to the Humber, in which at low water this rock is said to appear. The stone is chiefly used for repairing roads, but in some places is burnt to lime; its general character is remarkably ochreous; two beds are distinguishable in it—the upper sandy, and decomposing into round balls; the lower decidedly oolitic, and variegated internally with patches of deep blue. In the fossils of this whole range, we found nothing indicative of the upper oolites, but obtained several shells characteristic of the inferior;* and we do not hesitate to consider it as the continuation of that rock passing the Humber from Lincolnshire.

A little further to the east on the left hand side of the same road, we met with the third range which we had designed to examine exposed in a line parallel to the former in sand pits, at North Newbald, South Newbald, and South Cave; it appears also on the road about half-way between the last-mentioned places. Further south we were not able to trace it, but think it probable that the hill at Elloughton belongs to the same stratum, which would bring its course down to the Humber: at North Newbald it passes under the chalk without any interval between the two rocks. The character of this stratum is well shown in the section exhibited at the latter place, of which a drawing has been made by Mr. Phillips; it consists of loose sand abounding in casts of shells, and containing masses of a hard calcareous stone marked by numerous remains of gryphœa dilatata. Mr. Phillips at once recognised in it the Kelloways stone, such as it occurs at the base of Scarbro' Castle Cliff, and found in it the peculiar ammonite of that rock. (A. Calloviensis of Sowerby.)

The remaining object of our investigation in pursuing the line from west to east across the strata to the chalk, was the clay which appears in Elloughton Dale under the red chalk. This stratum we traced southward as far as Welton, and northward to Brantingham, and there can be no doubt that it is the same bed which throws out the springs at Newbald, near which place its outer edge must pass under the chalk, for there the chalk rests immediately on the subjacent Kelloways rock. The best exhibition of it is in Elloughton dale, where there is a section of the chalk marle, red chalk marle, and blue clay: the red chalk marle abounds here with the same small belemnites as it contains elsewhere, but in the blue clay there are none of these. I found in it one of the large belemnites which occur in the clay at Specton, and Mr. Phillips found two valves of the Ostrea deltoïdea (of Sowerby), with carbonised wood attached to them.

* Especially Terebratula spinosa (Smith).
Turritella (Sp. 1. Strat. Syst.)
Lima proboscidea.
Trigoniæ.

A similar belemnite was shown us from the clay at Brantingham, and we had heard of a similar oyster procured near Welton; at Welton we saw a large ammonite which had been found in a nodule in the same bed.

The similarity of appearances at Elloughton dale and Specton would lead to the supposition that the clay at those places belongs to one stratum; Mr. Phillips has found the Ostrea deltoidea also at Kirby Moorside, and it is probable that the whole substratum of the Vale of Pickering consists of the same clay. Its unconformity with the chalk, its position, as it should seem, above the coral rag, and the discovery of the Ostrea deltoidea, decide us to consider it as the Kimmeridge clay, overlying in the district which we are now describing, the coral rag, calcareous grit, and Oxford clay, which reappear to the northward.

Between the Kimmeridge clay and the soft aluminous chalk which forms the base of the Wolds, and which we consider as the chalk marle of Webster and Smith, no indications appear of any of the beds which intervene further south, except of the gault or Folkstone clay; the small transparent belemnite described by Lister seems to mark the red chalk marle as the representative of this bed.

Upon the whole then a section of the district which we have examined would probably exhibit a series of beds such as is described in the sketch accompanying this paper.

It only remains to add, that on our return we met with the red marle again a mile to the west of Shipton, and that at Holme, we observed the gravel to consist of the same fragments of sandstone and grit, mountain lime and slate, as in other parts of the Vale of York, but that at every point where we met with gravel near the chalk hills, it consisted of materials brought from a very short distance; and though the soft chalk pebbles were rounded, the gryphites and

flints were very little rubbed. In one of these beds of gravel we observed a seam which bore a remarkably black and sooty appearance; I examined the powder which occasioned this colour, and which formed a mamillary incrustation on some of the pebbles, and found it to be oxide of manganese. The method which I employ for detecting the presence of manganese is very simple. To the end of a platina wire I attach a little subcarbonate of soda and a particle of the substance to be examined, and hold it in the exterior flame of a candle either just above the luminous point, or on one side; if manganese be present, the melted bead becomes, on cooling, of a turquois colour; on immersing it into the visible flame, it loses this colour, and resumes it again when re-exposed to that portion of the flame which emits little light, and where the combustion is perfect.

Since this paper was written, inquiry has been made of Mr. Smith what his opinion was of the district above described, without any communication of the remarks which we had made; he replied that the sand pits at Newbald were of the Kelloways rock, and the limestone at Sancton of the inferior oolite; that with respect to the clay under the chalk hills, his observations did not enable him to form a decided opinion. Such a confirmation, as to the two former strata, of the views which we have advanced inclines us to place confidence in their accuracy. The principal deficiency in the foregoing account is in regard to the cornbrash, the fossils of which have been discovered at Scarbro' in their proper place; we were unable to find it here, but it may probably exist, as at Scarbro', in a thin bed, and be found hereafter between Newbald and Sancton. To future investigators, it will suggest itself also as a proper object of inquiry to follow the line of the Kelloways rock across the Humber, and complete the discovery of its course by tracing it through Lincolnshire.

Explanation of the Plate.

Fig. 1. South of Goodmanham, chalk upon lias.

a. White chalk.
b. Red chalk.
c. Lias clay.
d. Stony lias.
e. Lias clay.

Fig. 2. Elloughton Dale, chalk on Kimmeridge clay.

a. White chalk.
b. Red chalk.
c. Yellow parting.
d. Kimmeridge clay, with septaria.

Fig. 3. North Newbald, Kelloways rock.

a. Brown sand and stone.
b. White and yellow sand.
c. Sandy shelly blocks.

Fig. 4. Ellerker, inferior oolite.

a. Sandy beds.
b. Irony balls in sand.
c. Oolite in oblique laminæ.
d. Oolite with blue "cores."

Fig. 5. North Cliff, lias beds.

$\left.\begin{array}{l} \textit{a.}\ \text{Compact lias stone.} \\ \textit{b.}\ \text{Shelly lias.} \\ \textit{c.}\ \text{Thin-bedded lias.} \end{array}\right\}$ lying in clay.

Article VII.

Report of the Committee appointed by the Council of the Astronomical Society, for the Purpose of examining the Telescope constructed by Mr. Tulley, by Order of the Council. (Communicated by the Council of the Astronomical Society.)

Your Committee in making this report, before entering on the immediate subject of it, think it will not be unsatisfactory to the Council if they recapitulate briefly the circumstances, which have led to it, by way of presenting in one view the history of the telescope in question.

So long ago as the 29th of September, 1821, a communication was made to the Council of the Society, by M. Reynier, of Neufchatel, in Switzerland, on the part of an artist of the name of Guinand, resident in that neighbourhood, stating him to be in possession of a process for making discs of flint glass, fit to be employed in the construction of object glasses for achromatic telescopes, and free from the defects which have hitherto given so much annoyance to opticians—and of any required size even as far as twelve inches and upwards—and claiming for him a priority of invention before his former employers Messrs. Fraunhofer and Utzschneider of *Benedictsbeurn*, in case of dispute.

The extreme difficulty experienced by our artists in procuring discs of flint glass of even very moderate dimensions, had long been severely felt, and the first prospect of an opening afforded for the cessation of this inconvenience, which bore so heavily on the progress of practical astronomy, could not but excite the attention of the Council. Declining, however, to constitute themselves judges in any dispute respecting the invention of the process, they contented themselves with inviting M. Guinand to submit specimens of his performance for examination, and making such further inquiries as to his prices and his ability to furnish a regular supply as the nature of the case, and the wants of British artists, appeared to call for.

This invitation was at first, however, very unsatisfactorily complied with, the specimen sent being merely sufficient to authorise a judgment as to the quality of the glass with respect to refractive and dispersive power, but too small to justify any conclusion as to the real merit of the process by which it had been made. The principal was a disc of about two inches in diameter, of which an object glass was immediately constructed by Mr. Tulley, and of which that artist and Mr. Dollond reported as favourably as the trifling magnitude of the specimen would permit. The inquiries of the Council too were answered in a manner hardly more satisfactory, M. Guinand appearing chiefly desirous of disposing of his stock of discs on hand, and that a very limited one, at a tariff annexed, and of obtaining a pecu-

niary compensation for his secret, rather than of continuing the manufacture. A Committee was therefore appointed, consisting of Messrs. Gilbert, Herschel, and Pearson, to examine the telescope, and thereon to report on the propriety of purchasing a larger specimen for further trial. The report of this Committee will be found on the books of the Society. (A copy is subjoined.)

A copy of this report was immediately forwarded to M. Guinand, through M. Reynier. Meanwhile, however, other and larger specimens, consisting of fragments of irregular figure, were transmitted, and finally, a disc of $7\frac{1}{4}$ English inches in diameter was placed by Messrs. Guinand and Reynier, at the absolute disposal of the Society to examine without reserve, and to report on as its merits should appear to require. It is this disc, which forms the chief object of the present report, the fragments, though considerable, and apparently of good glass, being still not large enough to excite much interest, or call for particular attention. This, however, was not considered the case with the disc (one of such size fit for use, being very uncommon, if not at that time unique in England), and having now, from the free and unreserved mode of its communication, the means of putting the pretensions of the artist to a fair practical test, it was considered by the Council as a duty, due from them to the public, to take every adequate step for that purpose. The disc was, therefore, placed in the hands of Messrs. Dollond and Tulley on the 14th of November, 1823, with directions to take every proper means for ascertaining its efficiency for optical uses, and it was finally agreed between those gentlemen that Mr. Tulley should undertake to form it into the concave lens of an achromatic object glass of 12 feet focal length.

This has accordingly been done, but considerable difficulty was experienced in obtaining a disc of crown glass sufficiently homogeneous to match it, and it is evident that this was essential to the object in view, both glasses being of equal importance. A disc of French plate glass at that time in his possession was first tried, but after working it with all possible care the combination turned out defective, and the telescope resulting, though not a bad one, proved inferior to the high expectations, which had been formed of it. An artist of ordinary perseverance would, perhaps, have been discouraged by this indifferent success in a trial on so large a scale; and the glass, without further examination, would have been condemned; but Mr. Tulley, with a zeal and constancy for which he is entitled to much credit, still conceiving that the fault might mainly lie in the plate glass, resolved on commencing anew. Having, after much research, obtained another fit disc of the less refractive medium, being English plate glass, he again set to work, *ab initio*, refiguring the flint glass, and the object glass now to be reported on is the result of these his second labours. These circumstances your

Committee think it necessary to mention by way of accounting (and in their minds satisfactorily) for the long interval elapsed from the first reception of the disc to the final completion of the object glass.

In the state in which it has been submitted unreservedly to their inspection, at Mr. Tulley's house at Islington, mounted in a temporary wooden tube, and on a stand of very convenient construction for astronomical uses, its clear aperture is six inches and eight tenths, and its performance has proved in the highest degree satisfactory. It has been tried by us on various objects, both by day and by night; among the latter, the planets Jupiter and Saturn, several of the most delicate and difficult double stars, such as *Polaris*, γ *Leonis*, ζ *Cancri*, ω^2 *Leonis*, &c. as well as some of the small resolvable nebulæ in the constellation *Virgo;* severe tests these of the performance of a telescope, under magnifying powers from 200 to 700.

The examination of a bright object on a dark ground, as a card by day-light, or Jupiter by night, with high magnifying powers affords, as is well known, the severest test of the perfect achromaticity of a telescope, by the production of green and purple borders about their edges in the contrary case. The telescope in question bears these tests remarkably well, and is certainly more achromatic than usual, a circumstance depending not merely on the nice adjustment of the foci, but on the quality of the flint glass mainly. This might not have been expected (according to a remark of Dr. Brewster) from the high refractive and dispersive power of the glass, but the fact is undoubted.

The destruction of the aberration of sphericity in an object glass when thoroughly accomplished, even with the best materials, is the strongest proof of the goodness of its workmanship; but except the materials be good, no excellence of workmanship will destroy that irradiation which surrounds the image of a star with lines of light darting from it as a centre, and which fills the field with loose dispersed light. The object glass in question is perfectly free from the latter defect, and almost entirely from the former. The rudiments of rays may, indeed, be traced in interruptions of the regular contour of the rings which surround the spurious discs of large stars, and which arise from the interference of the rays grazing the edge of the aperture. Portions of these rings are wanting or very faint, and other portions somewhat stronger; so that in some directions the outlines of rings of several orders may be traced,—in others only those of the first and second. This defect was distinctly perceived in the image of γ *Leonis*, with a power of 220, giving it the appearance of having an excessively faint small star, almost close to the large one of the double star; but the inconvenience is so slight, that without critical attention, its existence would not be

suspected. It is no way offensive, and can certainly not be called a serious defect, and might arise from imperfect adjustment. In every other respect the definition of this star, with the power mentioned, was excellent.

ω^2 *Leonis* is one of the most difficult double stars in the heavens. With 220 it was seen elongated: with 700 it was distinctly seen to consist of two discs in apparent contact. With this high power, a slight degree of diffusion in the light of the stars was perceptible, but on the whole the performance of the telescope was extremely good.

ζ *Cancri* was examined with 300, 450, and 700. With the lowest power it was seen triple, very beautifully defined, and the close stars distinctly separated. With 450 they were well separated, and the black interval distinctly seen. With 700 the separation remained perfectly distinct.

A minute star was suspected near α^2 *Cancri;* but on comparing the diagrams made of it with its real position, it could not have been the true companion of that very difficult double star, which to be perceived requires the full power of reflectors of the largest class.

The companion of *Polaris* was of course perfectly well seen.

The light of this telescope is, however, amply sufficient for showing the nebulæ of Sir W. Herschel's first class. Several of these were examined, and the high degree of concentration of the rays in the focus, arising from the absence of aberration, proved very valuable, and was evidently marked in the resolvable appearance exhibited by them.

Saturn was shown with great distinctness, the division of the ring, and the three interior of the old five satellites being plainly seen. A satellite on the body of Jupiter was also seen as well as its shadow; and the planetary discs of the other satellites could not be mistaken for spurious ones.

Your Committee consider that the facts above detailed speak sufficiently for themselves as to the excellence of the telescope to render comments or praise on their part superfluous; but they cannot close this report without observing once more on the great pains bestowed on its workmanship by Mr. Tulley, and his address in availing himself of the resources of his art in operating on a material which might certainly in the beginning be regarded as highly unpromising.

(Signed) G. Dollond.
J. F. W. Herschel.
May 12, 1826. W. Pearson.

The report alluded to above :—

At a meeting of the Committee of the Astronomical Society for reporting on the propriety of purchasing specimens of M. Guinand's glass for further trials, held March 17, 1823.

The telescope constructed by Mr. Tulley was produced and

examined, and his letter, and that of Mr. Dollond, read; as also such parts of a correspondence between the Foreign Secretary and M. Reynier, as appeared necessary.

It was then resolved that it appears to your Committee that M. Guinand has not answered in a satisfactory manner to the inquiry put to him through M. Reynier, whether he will engage to furnish the London artists at a reasonable price, with flint glass fit for their purposes, inasmuch as he holds out no assurance of a regular supply, and has actually but a very limited quantity of his glass to dispose of, and that principally in discs not exceeding four inches in diameter; and your Committee conceive that no degree of excellence in individual specimens would authorise them to recommend their purchase by the Society, unless supported by such assurances of constant supply as would render it a matter of public interest.

Davies Gilbert.
J. F. W. Herschel.
April 11, 1823. Wm. Pearson.

Article VIII.

Abstracts of Papers in the Philosophical Transactions for 1825, *on the peculiar Magnetic Effect induced in Iron, and on the Magnetism manifested in other Metals, &c. during the Act of Rotation.* By Messrs. Barlow, Christie, Babbage, and Herschel.

1. *On the temporary Magnetic Effect induced in Iron Bodies by Rotation.* By Peter Barlow, Esq. FRS. (With a Plate.)

Mr. Herschel, in a conversation with Mr. Barlow on subjects connected with magnetism, had inquired what effect Mr. B. thought might result, from giving to an iron ball a rapid rotation. The subject, however, dropped, and did not again occur to the author, until recalled to his mind by some speculative views which he entertained as to the cause of the rotation of the earth's magnetic poles, &c. Being urged to the inquiry by these views, as well as by Mr. Herschel's query, and encouraged by the circumstance that Mr. Christie had found a permanent change in the magnetic state of an iron plate by a change of position on its axis, Mr. Barlow resolved to put the idea to the test of experiment, on a scale that should decide the question in the first instance. He describes the method he pursued, and the results he obtained, in the following terms:

"As soon as I had determined upon the experiment, I found an excellent opportunity of making the first trial, through the kindness of Generals Cuppage and Millar, of the Royal Artillery,

Fig. 3.

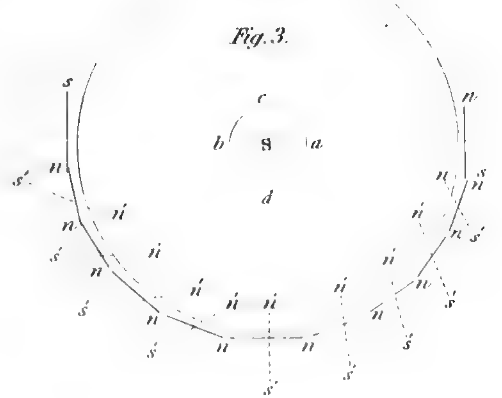


Fig. 4.

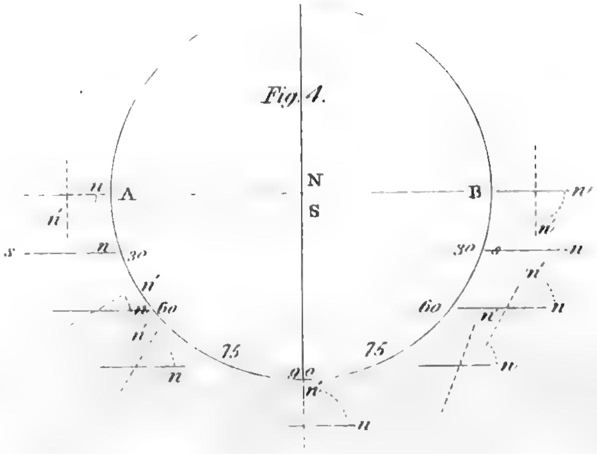


Fig. 1.

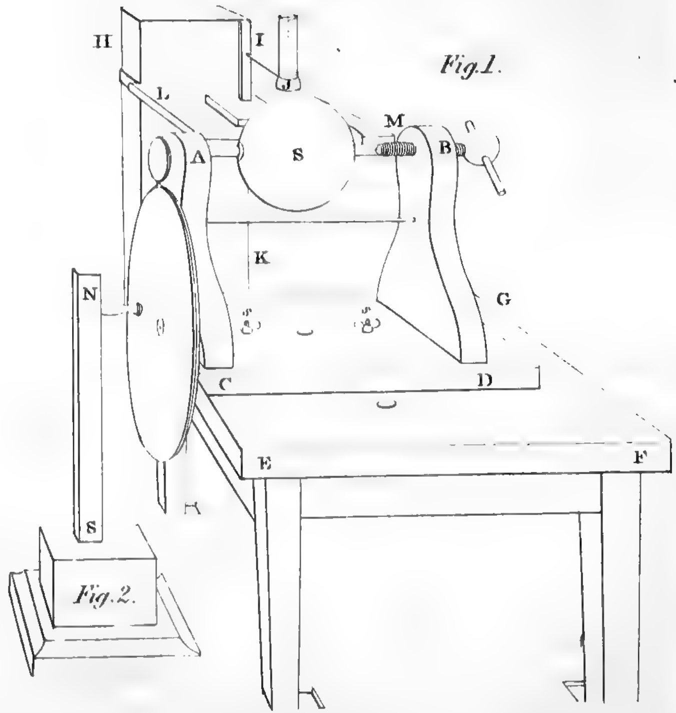


Fig. 2.

J. Shury, sc.

Engraved for the Annals of Philosophy, for Baldwin, Cradock & Joy, June 1, 1826.

who gave me permission to have a 13 inch mortar-shell fixed to the mandrel of one of the powerful turning lathes worked by the steam engine in the Royal Arsenal. This having been done, and the compass properly placed near the shell, I turned the shell slowly round, in order to ascertain whether in this case, as in Mr. Christie's, there were any effects depending on a change of position; but if there were any, they were so small in the cast-iron shell as not to have been rendered sensible with the small compass I employed. The wheel being now put in geer, the shell commenced its revolutions at the rate of 640 per minute, and the needle was deflected out several degrees, at which it remained perfectly stationary while the ball was in motion; but it returned immediately to its original bearing as soon as the motion ceased.

"I now inverted the motion of the shell, and the needle was deflected about the same quantity the contrary way, observing a similar steady direction as in the former case; but as before, it returned to its original bearing the moment the motion was discontinued.

"I afterwards found, that the needle being placed in different situations, its motion was reversed, although the direction of motion in the shell was the same; the amount of the whole deflection also differed very considerably according to the situation of the compass, its direction in some cases having been wholly reversed, while in others no perceptible motion was produced, although the rotation of the shell remained the same both in direction and in speed.

"I was, therefore, desirous of undertaking a regular set of experiments, in order to reduce the several apparently anomalous results to some certain law of action; and as the shell in question was rather too heavy for us to feel a perfect security, as to personal safety, when it was in rapid rotation, and, moreover, as its effects were larger than seemed necessary for the purpose, I now selected a Shrapnell shell of eight inches in diameter, which weighed only 30 lbs. and chose another lathe, whose axis was nearly north and south, that in the former instance having been east and west. I had also a table made with a circular hole in it, which I could place at any height above, below, or about the centre of the ball; I could also set my compass on any azimuth on the same, and observe the effects of the direct and reversed motion; but after several days' observations, I found the results so complicated, and the needle so much influenced by the iron-work of the lathe and other machinery, that it would be useless to proceed, unless I could contrive to produce the rotation out of the way of any disturbing cause of the kind above-mentioned."

This Mr. Barlow was enabled to accomplish by means of a machine delineated in Plate XXXIX, and thus described:

"A B C D is a strong wooden frame, resembling that of a common electrical machine, the shell S being hung in the same manner as the cylinder; the axis is made in two parts of gun metal, and very strong; *s s* are two strong screw bolts and nuts, which were used for fixing the frame firmly to the top of the table, the bolt passing through from below. E G F is a substantial table with its feet sunk into the ground, and the floor of the room cut away where they passed through, in order to prevent any effect of shaking on the stand carrying the compass.

"The stand consisted of an upright pedestal filled with sand, to render it steady, and to this was fixed the table M L, with a semicircular hole cut in it, so that it might be placed near the shell. This table might be elevated or depressed at pleasure, and it was divided into the points, quarter-points, &c. of the compass.

"By means of different holes bored in the top of the table, the machine might be placed N and S, E and W, &c. at pleasure, and the motion of the shell be inverted by turning the handle to the right or left. The large wheel is six times the diameter of the small one; and as it might easily be turned twice in a second, the number of revolutions of the shell were gradually about 720 per minute. The little apparatus seen above the shell is a small stand and sliding wire, carrying a common lamp-glass, in which a very small dipping-needle was suspended by silk; and when the lamp-glass was out of the ring, the latter served for setting the horizontal needle on, so as to bring it over any required point of the shell. It should be observed that the pedestal was moveable, and might therefore be placed on either side of the machine. The stand and upright figure 2, is one of two large magnets ultimately employed for neutralizing the needle.

"The machine being thus prepared, I screwed it down; first with its axis in the magnetic meridian, and then placed the compass successively at the several points on the table all round, and registered the deviation produced at each, with the motion of the shell direct and reversed. I then removed it, and placed the axis east and west, and again registered in the same manner; but the results were very irregular with respect to quantity. Although I obtained some uniformity *regarding direction only*, viz. in both cases I found four points of change at about 30° from each extremity of the axis, or four points of non-action. For example, when the axis was in the meridian from N 30° E to N 30° W, the motion of the needle arising from the rotation was made to the right. From N 30° W to S 30° W to the left. From S 30° W to S 30° E to the right. From S 30° E to N 30° E to the left; the direction of motion in the shell being the same; with the direction of motion reversed, the deviation was reversed also. While at these four points themselves, the needle

had no motion. I tried also a variety of other positions, but I could obtain no such results as to lead to a concise expression of the effect, and for this reason I shall not trouble you with the detail of them.

"It at length occurred to me, that the reason of my failure arose from the compound influence under which the needle was placed, viz. that of the iron ball and of the earth; I therefore now neutralized it from the effect of both, by means of magnets properly disposed, adjusting it always before the rotation to a direction tangential to the ball, so that whatever effect was produced at each point, might at least become decided as to its direction. I now immediately arrived at that kind of general law I had been in search of; for I found when things were thus arranged, that whatever might be the direction of the axis of rotation, if the motion of the ball were made towards the needle, the north end of the latter was attracted; and if from the needle, the north end was repelled by the iron, the points immediately in the axis (when of course the motion of the shell was parallel to the needle) being neutral, or those at which the change of direction took place; in other words, if the motion of the shell continue the same, and the compass be successively placed all round the ball, in that semicircle (from one axis to the other) in which the motion is towards the needle, the north end approaches the ball, and in the other semicircle it recedes, or the south end approaches; the points of non-action being in the two extremities of the axis, and those of maximum effect in two opposite points at right angles to the axis; in which two latter, the needle, when properly neutralized, points directly to the centre of the ball.

"This will be perhaps better understood by reference to fig. 3, where S is the shell, *a b* its axis, and *n s*, *n s*, &c. the needle in its various positions prior to the motion, and *n′ s′*, *n′ s′*, &c. its direction as resulting from the motion; the rotation of the shell being from *c* towards *d*.. of course with the rotation reversed, the effect will be reversed also."

The author here observes, addressing Mr. Herschel, "Now this effect you will, I think, find to be perfectly consistent with the view you have taken of the subject, in your letter of Jan. 13, where you say in reference to your former query, and to the views I then entertained, 'I should rather have expected a diminution of the magnetic polarity, commensurate to the rapidity of rotation and a change in the direction of the magnetic axis of the globe, from parallelism to that of the earth, to a position somewhere intermediate between that and the axis of rotation, but approaching nearer the latter as the velocity increased, &c.'

"The fact is, that the needle in my experiments being under no influence, prior to the rotation, from either the iron or the earth,

the direction which it takes up in consequence of the motion, enables us to discover the precise direction of the new forces thus impressed upon the shell, and it will be seen immediately to indicate a polarization of the latter in the direction *c d*; that is, in a direction perpendicular to the axis of motion, and to the plane passing through that axis and the actual poles of the ball.

"You will of course understand that I do not mean that such a polarization actually takes place; I mean merely that the cohesive power of the iron is such, as to resist in a certain degree the inductive powers of the earth, whereby the magnetic forces are changed, as you have suggested, from their original direction, parallel to the magnetic axis of the ball, into a position oblique to it, which oblique forces being resolved into two, the one parallel to the original axis, and the other perpendicular to it, and the former being nearly neutralized by the magnets used for the purpose in the first instance, the perpendicular forces will act upon the needle in the same manner as if the ball were really polarized in the direction above alluded to.

"Having got this view of the subject, I soon found that many of my former results, which appeared to have scarcely any conformity among themselves, were perfectly consistent with this hypothesis: of these the experiments given above, before the needle was neutralized, may be mentioned. In these I found the point of change to be at about 30° on each side of the axis, so that the arcs in which similar effects were produced were divided into the unequal portions of 60°, 120°, 60°, and 120°, which appeared to be anomalous; but according to the view now taken of the subject, this is perfectly consistent; it is precisely what ought to happen according to the law, tan. dip. = 2 tan. mag. lat. and which actually takes place on the earth. That is, in passing from the magnetic equator 30° towards the pole, the dipping-needle has actually described a quadrant, as referred to its position at the equator; and it would describe a quadrant, in an opposite direction in going 30° towards the other pole; so that in passing through 60° the needle is actually inverted; but if we start from mag. lat. 30° through the pole, we must pass through an arc of 120° before the direction of the needle is inverted, and the same in the other half of the meridian; and in like manner by referring the motion of my needle as induced by the rotation of the shell to its original magnetic direction, it is obvious that I ought to have found, as I actually did before I was aware of the cause, a point of change at 30° distance on each side of the meridian passing through the axis; which meridian, as respects the induced power, is actually the equator of the new magnetic sphere.

"To render this more obvious, let us refer to fig. 4, in which A B represents the axis of rotation of the shell, the black lines the needle in its natural direction, and the dotted lines the

direction the needle has a tendency to assume according to the law above-named, in consequence of the magnetism impressed by the rotation in the line *n s*. Beginning at the point A, if we say the motion is from left to right, that is from *n* to *n′*, it will be from right to left at 60°, 75°, 90°, &c. till we arrive again at 30°; at this point as at the former the new power is exerted in the actual direction of the needle, and if it were greater than its natural directive power, it would wholly invert it; in this case it would pass to either hand; but as the new power cannot invert it, it has no tendency to deflect it, and it therefore remains stationary. Thus one of the results which was at first the most perplexing, serves to confirm the law we have established.

"On similar principles, if we conceive a circle passing vertically from 90° to 90°, and if the needle be perfectly neutralized at different positions in this circle, and rendered parallel to the axis at each, then in every case the needle will have a tendency to take up a position directly at right angles to the axis of the shell, and it will point in opposite directions at certain parts of this circle: thus, if to fix the idea we conceive the axis to be in the meridian, and the motion of the shell from west to east, then at the east point of the horizon the needle will point to the west, and it will do the same at all points between the horizon and an altitude of 60°; beyond this, the north end will point to the east till we have passed the zenith 30° on the west side; and then again from this point to the west horizon the north end will again point to the west; and similar changes will take place below the ball. This, which is a necessary consequence of our hypothesis, is completely verified by experiment.

"It is presumed, that what has now been stated is sufficient, without referring to any further experiments, to establish the principal fact adverted to in this letter, viz. that when any iron body is put in rapid rotation on any line not coinciding with its magnetic axis, a temporary derangement takes place in its magnetic powers, which in its effects is equivalent to a new axis of polarization perpendicular to the plane passing through its axis of polarization and rotation."

(*To be continued.*)

Article IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 27.—A letter from Benjamin Bevan, Esq. to Dr. T. Young, For. Sec. RS., was read, describing some Experiments on the Elasticity of Ice.

In this communication, Mr. Bevan first refers to some experiments on this subject, made by him in the year 1824, in which the modulus of elasticity of ice appeared to be above 6,000,000 feet, with the view of comparing their results with those he obtained last winter from experiments on a larger scale. In the latter, a prism of ice 100 inches long, 10 inches wide, and of the mean thickness of 3·97 inches, being tried with weights up to 25 pounds, exhibited a deflection of $\frac{206}{1000}$ of an inch, which gives for the modulus 2,100,000 feet: and on examining the calculations from his former results, the author detected an error in the reduction, by the correction of which he found them to indicate the same modulus as his more recent experiments. In these, ice of various thickness, from one and a half inch to four inches, and whether tried on the water, or taken out and tried in the same way as wood or metal, gave by computation the same modulus, as in the instance just related.

Mr. Bevan states that Dr. Young, in his valuable lectures, has given 700,000 feet for the modulus of elasticity of water, computed from Canton's experiments on the compression of that fluid; but taking Canton's results under another point of view, Mr. Bevan finds the modulus they indicate to be 2,178,000 feet; which very nearly agrees with the result of his own experiments on the elasticity of ice.

In a note to this paper, Dr. Young states his opinion that the same modulus should be yielded by solids and by fluids; and he also refers to an experiment of his own, made some years ago, on the sound given by a piece of ice, in which the modulus did not appear to be greater than 800,000 feet.

A paper was also read, on the application of the Floating Collimator to the Dublin Circle; by John Brinkley, DD., FRS.: communicated by the Board of Longitude.

Dr. Brinkley details, in this paper, the results of an examination to which he has subjected Capt. Kater's Floating Collimator: they are favourable in the highest degree to the utility of the instrument, which belongs, Dr. B. says, rather to a future age of practical astronomy, than to the present. He finds the

Collimator to be applicable to any circle, without introducing any errors whatever of its own.

May 4.—Colonel Mark Wilks was admitted a Fellow of the Society; and the following paper was read:

On the means of facilitating the Observation of Distant Stations, in Geodesical Operations; by Lieut. T. Drummond, Roy. Eng.: communicated by Lieut. Col. T. Colby, FRS.

A committee of the House of Commons, having recommended to his Majesty's government in 1824, the accomplishment of a new survey of Ireland, which circumstances rendered it expedient to effect as speedily as was consistent with accuracy, Lieut. Col. Colby, whilst engaged in active preparations for the survey, entrusted to the author the contrivance of means for obviating the delay which usually occurs in connecting the stations in triangulation in this country, from the frequently unfavourable state of the weather not permitting the ordinary signals to be seen from distant stations. The delay from this cause, which had been experienced in the Western Isles of Scotland, gave reason to apprehend that such would be the case, to a still greater extent, in Ireland.

To remove this inconvenience, as far as day-observations were concerned, Lieut. Drummond had recourse, in preliminary trials, to *tin-plates,* as substitutes for regular Heliostats; and the advantages derived, from applying, even in this rough way, the principle of reflection, as suggested by Prof. Gauss, led to the invention of an instrument described in the paper, which was used with much benefit last season, in the survey of Ireland.

Some method of connecting the stations during the night was also desirable, in order still further to expedite the prosecution of the survey. For this purpose, Bengal and white-lights had formerly been employed by General Roy, but the use of them had given way to that of Argand lamps, their light being concentrated, and reflected towards the observers, by a parabolic mirror. These, however, had been found to answer but imperfectly; and Col. Colby and Capt. Kater, when connecting the meridians of Greenwich and Paris, in 1821, with M.M. Matthieu and Arago, employed the light of an Argand lamp with four concentric wicks, concentrated by a lens.

This apparatus, however, though well-adapted to light-houses, for which purpose it had been originally devised by M. Fresnel, was in many respects objectionable in geodesic operations; and the parabolic reflector still appeared to be the most eligible means of concentrating the light, from whatever source that might be obtained.

With the view of obtaining a powerful light, Lieut. Drummond first tried various pyrotechnical preparations, and afterwards the combustion of phosphorus in oxygen gas, but he found,

in all these, that the light was ill-defined, and otherwise unsuited to the object in view. He then had recourse to the light emitted by some of the earths and metallic oxides, when ignited by the flame of alcohol, urged by oxygen gas. Taking the light of the brightest part of the flame of an Argand lamp as unity, and effecting the comparison by the method of shadows, he found the light given out by quicklime, when under this treatment, to be equal to 37; that emitted by zircon 31; and that by magnesia 16: oxide of zinc was also tried, but it quickly wasted away, and gave out less light even than magnesia. The best kind of lime for the intended purpose is chalk-lime, which admits of being turned readily into small balls, having a stem, and to which the regularity and truth of surface can be given, which are essential to the production of the well-defined image, necessary for the perfect use of the contrivance in geodesical operations.

This lime, when the experiment is most successful, emits a light exceeding 83 times that of the brightest part of the flame of an Argand lamp. In the focus of the parabolic reflector, at the distance of 40 feet, it is almost too dazzling to look at.

From the perfect success which attended the employment of this mode of illumination, on one occasion in Ireland, last year, it is expected that it will enable the officers employed in the survey, to complete with celerity, and in the most satisfactory manner, the connection of distant stations. Lieut. Col. Colby purposes to connect, by means of this invention, the meridian of the observatory on the Calton Hill, at Edinburgh, with that of Dublin, taking Ben Lomond as an intermediate station; one side of the triangle in which operation will measure above 90 miles. Other applications of it are also contemplated.

Lieut. Drummond found that a mixture of hydrogen and chlorine gases, exposed to the light given out by the lime, was converted into muriatic acid; and that when the light was decomposed by a prism, the violet ray it contained had a marked effect on chloride of silver.

A note addressed to the author by Mr. Herschel was annexed to this paper, stating the results of a cursory optical examination of the light emitted by the incandescent lime. It contains all the usual rays, and three of them remarkable in quantity and quality: viz. a red, intermediate between the red and orange of the solar spectrum, but nearer to the latter; a yellow; and a green. Mr. Herschel points out as a curious fact, that a red of the above character should be yielded by lime itself, whilst the colour given to burning bodies by the combinations of that earth is a *brick-red*, very distinct from the hue imparted by strontian, which is of a carmine tint.

May 11.—Sir J. S. Copley, His Majesty's Attorney-general, and L. A. De la Chaumette, Esq. were admitted Fellows of the Society: A paper was read, On the Production and Formation

of Pearls; by Sir E. Home, Bart. VPRS. It is stated, in this communication, that pearls originate in the blighted ova of the shell-fish that produce them, and that they afterwards receive a coat of naker, when the interior of the shell receives its annual supply.

The reading was commenced, of a paper, On the Burrowing and Boring Marine Animals; by Edward Osler, Esq; communicated by L. W. Dillwyn, Esq. FRS.; and the Society then adjourned to the 25th of May.

PROCEEDINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN, AT THE FRIDAY-EVENING MEETINGS.

April 28.—Mr. S. Solly read a paper, on the Porphyry of Christiania, illustrated by a numerous and curious collection of specimens, from Professor Esmarck, and by drawings and engravings.

Mr. Jopling exhibited and explained in the library, his Septenary system of lines, produced by continuous motion. He showed the use of the instruments which had been constructed, for the application of the system to useful purposes.

Mr. Clowes sent a set of types, plates, and impressions, relative to his method of printing music in type. The printed sheets were very clear and correct; and it is in these two points that the advantage of the plan consists.

May 5.—Mr. Faraday explained in the lecture-room, the investigations of Mr. Hennel and himself respecting the sulphovinic and sulpho-napthalic acids, and of their salts; the peculiar powers of hydro-carbon in these compounds was pointed out, and the methods of obtaining the acids; their characters being at the same time illustrated.

On the library-table lay Mr. Perkins's book of patterns, produced by eccentric-lathe-turning, and one of his steel plates and rollers.

May 12.—The subject of the evening was Lieut. Drummond's Station Light, an account of which has been read before the Royal Society; to our report of which we refer. Mr. Faraday explained the nature and particular points of the application, from the lecture-table, and then the light was shown in the reading-room, and compared with that of an Argand lamp, both being placed in parabolic mirrors. Mr. Drummond's light immensely surpassed in brilliancy the Argand lamp.

May 19.—Mr. Turrell entered into the history of steel engraving, and described the various processes by which steel was prepared for the engraving; the precautions in decarbonizing and carbonizing it, in hardening and softening it, were stated; and the way was prepared for the process of engraving on steel, to be explained some other evening.

Mr. Ritchie, of Nain, brought forward two of his new and ingenious photometers; and Mr. Howship produced an in-

teresting specimen of Burmese art, being an edict written upon a varnished and highly-gilt leaf.

ASTRONOMICAL SOCIETY.

April 14.—The following Address was delivered at a Special General Meeting of the Astronomical Society of London, held this day, on presenting the Gold Medals to J. F. W. Herschel, Esq., J. South, Esq., and Professor Struve, by Francis Baily, Esq., FRS. LS. & GS. MRIA., and President of the Society.

The Members of the Astronomical Society are convened together this evening for the purpose of witnessing the distribution of the Medals, which have this year been awarded by the Council, agreeably to the powers vested in them for that purpose. The subject, which has called for this public expression of their opinion and approbation, is that of *Double Stars;* which has been pursued with uncommon zeal and energy by three distinguished members of your body.

The history of this particular branch of astronomy is but of recent date. For, it cannot be unknown to any of you that this subject occupied a considerable portion of the time and attention of our late illustrious President, Sir William Herschel; and that, in fact, it was he who first directed the attention of astronomers to this important branch of the science; having himself commenced and carried on, with great ability and diligence, a minute survey of the heavens, for the express purpose of detecting those almost imperceptible combinations of stars, which had hitherto escaped the observation of ordinary observers.

Assisted by his own inventive genius, and the labour of his own skilful and unerring hand, he contrived and brought to perfection telescopes of a size which may be truly termed *gigantic,* and possessing powers of vision and penetration far superior to any that had ever yet been used by astronomers: and with which he made those astonishing and remarkable discoveries that have filled the contemplative mind with wonder and admiration.

It did not escape the sagacity of this illustrious astronomer that these important discoveries, which he was the first to disclose to the world, might be made conducive to the investigation of the *parallax of the fixed stars:* a subject which has, from the earliest period, occupied the attention and curiosity of astronomers. And it was, in fact, this consideration that first led him to the pursuit of this important branch of astronomy: but this object was soon lost sight of, in the singular and remarkable phænomena which he afterwards brought to light.*

* Indeed the obvious use which might be made of such observations had occurred to Galileo, who first suggested the idea that the apparent distance of two apparently contiguous stars might perceptibly vary according to the position of the earth in its orbit. But, his theory was founded on very imperfect and unsatisfactory data: and he himself made no progress in the solution of this important problem.

Before he commenced his observations, however, he was desirous of ascertaining what other astronomers had done before him in the same pursuit. But, not having the facility of reference to many works, he himself (as he emphatically expresses it) opened the Great Book of Nature, and explored that vast and splendid volume, as the best catalogue that he could find for the occasion. At the time that he began his important and interesting inquiries, he was not aware of more than *four* stars that came under the description of double stars: yet, with this small stock he began his pursuit; and, in the course of a few years, formed a catalogue of 269 double and triple stars, which he presented to the Royal Society, and which is published in the Philosophical Transactions for 1782. In this Memoir, and in all his subsequent ones, he gave not only the *Distances* between the two stars, as measured by various methods, but also the *Angle of Position*, or the angle formed by the parallel of declination, and an imaginary line joining the two stars. These records have now become of considerable importance, as enabling future observers to compare their results, and thus determine the change which those quantities have undergone during the interval that has elapsed since they were made.

Ever ardent in the cause of science, this distinguished astronomer followed up his favourite pursuit by a second collection, consisting of 434 additional double stars; which was published in the Philosophical Transactions for 1785.

In the years 1803 and 1804 he communicated to the Royal Society "An account of the changes that have happened during the last 25 years, in the relative situation of double stars:" and it was in these papers that he first made known to the world those astonishing and important facts which have so justly excited the admiration of astronomers. In order to set this in a clearer light, I would remark that it had been hitherto a commonly received opinion, that the difference in the apparent magnitude of the fixed stars was caused by the difference in their distance from the eye of the observer: that a star of the first magnitude, for instance, was situated nearer to us than one of the second magnitude; and this again, nearer to us than one of the third magnitude; and so on in succession till we came to the smallest point visible in the most powerful telescopes: and moreover that those apparent combinations of stars, by twos or by threes, or any larger clusters (numbers of which present themselves to the eye of the observer) were merely the consequence of their lying nearly in the same line of vision, and that they were nevertheless separated from each other by an immense and immeasurable distance. But this, however much it may be true in some particular instances, is not universally the case: for, in the course of the observations alluded to

in the two papers just mentioned, the most remarkable and unexpected phænomena presented themselves. The apparent distances of many of the double stars were found to differ from what they had been at a former period; at the same time also that their angles of position were discovered to have undergone a perceptible variation, and evidently indicating a revolution round each other. This was the case whether the star had a considerable proper motion of its own; or whether it was apparently at rest with respect to the other stars around it: thus showing incontestibly that the two stars acted on each other agreeably to the universal law of gravitation.

In fact, in the language of Messrs. Herschell and South, "the existence of binary systems (in which two stars perform to each other the office of sun and planet) has been distinctly proved; and the periods of rotation of more than one such pair ascertained with something approaching to exactness. The immersions and emersions of stars behind each other have been noted; and real motions among them detected, rapid enough to become sensible and measurable in very short intervals of time." The most remarkable and regular instance of this kind is that of the double star ξ *Ursæ Majoris:* where the stars perform a revolution round each other in the short space of 60 years: and already three-fourths of the circuit has been actually observed from the first period of its discovery in 1781 to the present day. The double star *p Opiuchi* presents also a similar phænomenon, with a motion in its orbit still more rapid. In this case the two stars are very unequal in their magnitude. *Castor, γ Virginis, ζ Cancri, ξ Bootis, δ Serpentis,* and that remarkable double star 61 *Cygni,* together with several others, exhibit likewise the same progressive increase in the angle of position. The instances are indeed too numerous for me to enlarge upon in this place; and I allude to them merely with a view of drawing your attention to this important and interesting branch of the science.

These binary systems, it must be confessed, open a vast field of inquiry and speculation relative to the true system of the universe. The mind is lost in the contemplation of such immense bodies performing their revolutions round each other at such immeasurable distances. Our vast planetary system shrinks to a mere point, when compared to the orbits of these revolving suns. When we consider likewise the remarkable appearances exhibited by clusters of very minute stars, by nebulous stars and by nebulæ, and the singular changes which they seem to be undergoing, and which are too evident to admit of a doubt, and too important to be overlooked, we must confess that there is still much to learn in the science of astronomy. It is true that our late illustrious President has drawn some important inferences from those remarkable appearances which

he was the first to discover, and has advanced a theory relative to the system of the universe, which, whether it be realized or not, (and centuries must elapse before we can even approximate towards the truth of it,) must ever show the vigour of his bold and comprehensive mind.

The last production of this great man, relative to double stars, was communicated to this Society in the year 1821; and is inserted in the first volume of our Memoirs.

Such was the state of this interesting branch of the science at the time it was taken up by Messrs. Herschel and South. The singular and extraordinary changes that had been observed by Sir William Herschel in his review of the heavens in 1802 and 1804, had determined Mr. Herschel to follow up the intentions of his father, by a review of all the double stars inserted in his catalogues: and as early as 1816 he had commenced this arduous undertaking. Mr. South also being disposed to pursue the same inquiry, suggested the plan of carrying on their observations in concert: and, with the aid of two excellent achromatic telescopes, belonging to the latter, they employed the years 1821, 1822, and 1823 in this research. The result of their labours was presented to the Royal Society, and published in the Philosophical Transactions for 1824, at the expense of the Board of Longitude.

The number of double stars observed jointly by these two astronomers amounts to 380: and we may judge of their value and importance when we learn that the authors were more anxious to obtain accurate results, than to extend the field of their inquiries in the first instance. But, when we find that, even to obtain these results, many thousand measurements of distance and position were made, we must justly admire the patience and perseverance of the authors in this their laborious, but highly important pursuit. The remarkable phænomena, first brought to light by Sir William Herschel, have been abundantly confirmed; and many new objects pointed out as worthy the attention of future observers.

Whilst these important inquiries were carrying on in England, one of our Associates, Professor Struve, was engaged in similar observations at Dorpat in Russia. The result of his labours is contained in the several volumes of the Observations made at that observatory; and will be read with pleasure and advantage by every lover of astronomy.* The remarkable coincidence in most of the measurements made by M. Struve,

* Although not immediately connected with the object of this Address, I cannot omit this opportunity of noticing the labours of M. Amici on double stars. With some excellent and beautiful telescopes and micrometers of his own workmanship and construction, this indefatigable and careful observer has extended his examination to upwards of 200 double stars; and has detected motions in some of them, not yet noticed by other astronomers. It is to be hoped that his very valuable labours will be collected and published, for the benefit of science.

and those made by Sir William Herschel and afterwards by Messrs. Herschel and South (although with very different instruments and micrometers), confirms the general accuracy of the observations, and marks the degree of confidence that may be placed in measurements of this kind. Some slight discrepancies have indeed been observed on a comparison of the total results, and some singular anomalies have presented themselves: but these, so far from invalidating their accuracy, tend to give them greater confirmation, and may probably, at some future period, lead to the detection of some hidden law which regulates the motions of these remarkable bodies.

It is for these important observations and discoveries, and for the great zeal and talent displayed by these distinguished astronomers, in the pursuit of this interesting subject, that your Council has resolved to bestow on each of them the Gold Medal of the Society: and which I have now the honor of doing.

[The President, then addressing Mr. Herschel, said:] "In the name of the Astronomical Society of London, I present to you this Medal. You will accept it, Sir, as a mark of the deep interest which this Society takes in the object of your labors. Be assured that we are pleased to see (from the Paper presented to us this evening) that the subject still occupies your attention, and that it is likely to be pursued with so much energy and zeal, by one who can so fully appreciate the importance of such inquiries, and who is so competent to conduct investigations of this kind. We trust that you will have health and strength to pursue the path which you have thus commenced with so much honor to yourself, and so much benefit to science. Inheriting, as you do, those rare and exalted talents which distinguished your venerable and honored father, and aided by the resources of your own powerful and enlightened mind, you have already opened another and very interesting field of inquiry and research in this particular branch of astronomy, by proposing *a new method of applying such observations to the investigation of the parallax of the fixed stars:* a subject which cannot be fully appreciated till after the lapse of many years, and which we hope will not be lost sight of by those who are engaged in investigations of this kind. The name of Herschel, doubly connected as it thus is, with the history of astronomy, can perish only with all records of the science. The splendid example of the father has been emulated by the son: and you have the proud and enviable satisfaction of knowing that you will share the glory of his immortal name."

[The President next presented the Medal to Mr. South in a similar manner, and said:] "In presenting you with this Medal, Sir, I can only repeat the sentiments which I have just deli-

vered to your friend and fellow-labourer Mr. Herschel. The ardent zeal which you have always evinced in the cause of astronomy, the patience and perseverance which you have shown in conducting so many and so valuable observations, of no ordinary kind, and the skill and accuracy which you have displayed in those delicate measurements, are subjects that are duly estimated by this Society. Possessed of a princely collection of instruments, of exquisite workmanship and considerable magnitude, such as have never yet fallen to the lot of a private individual, you have not suffered them to remain idle in your hands, but have set an example to the world how much may be done by a single person, animated with zeal in the cause of science. Scarcely indeed have those labors issued from the press, for which this Society is now assembled to congratulate you, than they have been followed by a communication of others (now lying on the table) rivalling them in magnitude and importance; extending your examination to 460 additional stars (many of which are *new*), and confirming in a satisfactory manner the remarkable changes which had been noticed in your previous review. The subject which you have thus commenced with so much success, with so much benefit to science and so much honor to yourself, is as vast as it is important. The number of double and triple stars seems to increase with the attention that is paid to them: and already their amount is sufficient to appal an ordinary observer. Boldly pursuing the path of science your energy has, however, increased with your difficulty; so that few of these singular bodies have escaped your patience and penetration: and the Society hope and trust that the same talents will be exerted in a further prosecution of the subject. There is no doubt but that a careful examination and re-examination of these remarkable bodies will tend to throw some new and interesting light on the system of the universe: and it must ever be a pride and satisfaction to you to reflect that you have been instrumental in advancing the boundaries of this department of science, and that your own name will always stand conspicuous in the history of these discoveries."

[The President afterwards presented the Medal, in a similar manner, to Mr. Herschel, as proxy for Professor Struve, and addressed him as follows:] "Assure M. Struve of the lively interest which we take in all that is passing at the Observatory of Dorpat: that we admire the patience, the exertions and the address, with which he has overcome the difficulties he has had to encounter, in the progress of his discoveries: and that we look forward with confidence to a continuance of the same brilliant career in the cause of astronomy. Furnished, as he now is, with one of Fraunhofer's colossal telescopes, and thus

armed with the most powerful means, we anticipate the most successful results from his laborious exertions. Unconscious of what was going forward in this country, he had opened for himself a vast field of inquiry, which he has pursued with the most splendid success; and which places his name amongst the most celebrated of modern astronomers. The Paper which has been read to us, this evening, shows that his ardor is unabated: since he there announces the important fact of the observation of 1000 double stars of the first four classes (most of which are entirely new), and amongst which are 300 of the first class. To a mind, formed like his for the pursuit of science, little need be said to animate him to a continuance of his labors: but, it may be pleasing to him to know that we are alive to the progress of his discoveries: and I am sure that you will convey to him, in much better terms than I can do, the expressions of our esteem and admiration for his services in the cause of science;—services which assure us that the name of Struve will be imperishable in the annals of astronomy."

At this meeting was also read, "A Comparison of Observations made on Double Stars." Communicated in a letter to J. F. W. Herschel, Esq. Foreign Secretary to this Society, by Professor Struve, of Dorpat. Addressing himself to Mr. Herschel, M. Struve, says, "You may easily imagine with what interest I have perused the work on double stars, by yourself and Mr. South, and with what pleasure I found that, independently of one another, we have arrived at the same results and deductions. Although my instruments were formerly inferior to yours, with respect to measurements (as I could only observe differences of AR on the meridian, and angles of position with a 5-feet telescope of Troughton), they may be considered in an optical point of view equal to yours; viz. the 5-feet telescope of Troughton's to yours attached to the 5-feet equatorial; and the 8-feet one of Dollond to yours attached to the 7-feet equatorial; and after receiving the repeating micrometer of Fraunhofer, which I fixed to Troughton's telescope, every desideratum in this instrument was fulfilled."

M. Struve, however, found himself involved in some practical difficulties, until the arrival of Fraunhofer's large refractor, an instrument which, with respect to double stars, left him nothing further to wish; and he determined on a new examination of all the double stars observed before (whether by Sir W. Herschel, Messrs. Herschel and South, or himself,) as well as on a minute inquiry of the heavens from the north pole to $-15°$ of declination, with respect to these objects. He has now accomplished one-third of the labour, and has found 1000 double stars of the first 4 classes; *among which* 800

are new, and of these nearly 300 are of the first class. He extends the examination to all stars of the 8th and (8·9) magnitude.

The author, after detailing a few more preliminary remarks, enters into a comparison of many of his observations with those of Sir W. Herschel, and of Messrs. Herschel and South, pointing out many cases in which their coincidence is truly remarkable;—others in which there are discrepancies, evidently attributable to the relative or real motions of the stars in the intervals between the observations;—others in which the diversities seem occasioned by the instruments employed;—and others in which there are anomalies which do not, as yet, admit of explanation. This part of M. Struve's communication is not susceptible of abridgement.

GEOLOGICAL SOCIETY.

May 5.—The reading of Dr. Bigsby's paper, on the Geology of the Valley of the St. Lawrence, was continued and concluded.

In the first part of this paper, the author describes the general form of the country, in which are placed the great lakes of Superior, Huron, Erie, and Ontario, with a sketch of some of the rocks occurring in their vicinity.

He then examines the question of the present level of their waters, as compared with their ancient level, and enumerates many circumstances to prove that no recent alteration has taken place. He then endeavours to establish, that the Canadian lakes are monuments of the last flood, by the features of the country, the abrasion of its rocks, and the nature of the transported matter. He ascribes the shape of the Islands of Lake Superior, and of the Promontory of Keweewoonan, to diluvial denudation. The Manitouline gaps are adduced as effects of one simultaneous deluge.

Dr. Bigsby divides the debris of the St. Lawrence Valley into four classes:—1. Diluvial; 2. Messalluvial; 3. Alluvial; 4. Native.

1. The diluvium lies usually in extensive flattened heaps. Cape Tourment, 1800 feet above the level of the sea, is covered with it. Marine shells, of the genus *Saxicava,* are found in the Ottawa, 300 miles N.N.W. of Montreal.

The actual position of numerous primitive bowlders on the plains being south and south-east of their original sites, indicates that the flood proceeded from the north and north-west, or, in a direction *contrary* to the present course of the St. Lawrence. The trap of Montreal is found at Lake Champlain, and bowlders of tabular spar (one of which weighs 600 pounds) are traced to the west end of Ontario, where the Ophicalcic rock is also seen in broken masses. The chalcedonies from Lake Superior have been transported south-west to Lake Pepin. The

south and west shores of the Lake of the Woods are loaded with bowlders, whilst the opposite shore is destitute of them. These evidences of denudation are given as coinciding with the views of Saussure, De Luc, and Buckland.

2. The *Messaluvion* is presumed to have been formed in the intermediate state of the earth, which it assumed between its total submergence and its present form. At that period, central North America is imagined to have been occupied by one great lake; and the author's evidences to prove this, consist of, 1. The series of embankments, and 2. their being composed of adjacent rocks, and even of fresh-water materials. 3. Rolled masses of neighbouring lakes being reciprocally found in each other. 4. The peculiar nature of the sand and gravel, beneath the mould of the Valley of the St. Lawrence. 5. The mountain-barriers broken through for the passage of rivers and lakes. 6. The analogy of this supposed reservoir to those which have been traced in Germany, Scotland, &c. This enormous lake, or rather insulated portion of the ocean, must have extended, in the north, from Hudson's Bay to below Quebec: the eastern boundary being the Allighany range: the western the diluvial hills near the Rivers St. Peter, Red, and Missouri: whilst the waters contained therein must have stood at 1000 feet above the level of the sea.

That the fluid of this great reservoir was saline, is inferred from many genera of fish, *of marine origin*, being now the inhabitants of the lakes; which latter are presumed to have been converted into fresh-water by various operations of drainage, &c. Large fresh-water deposits are instanced, as occurring on lakes Huron and Simcoe, extending to Ontario and Erie. Some of the higher beds of these, in the interior, contain *Uniones*, like those of the present lakes: they are never in a fossil state, and are associated with *Planorbes, Physæ, Lymneæ, Melaniæ*, &c. The banks of the lakes are usually constituted of several steps or terraces, which the author attributes to the various depressions of the waters, occasioned by excessive injuries to the enbankments; but with respect to the great primary lake, he inclines to the belief of its reduction to a group by *one great disruption*. The chaudieres, or pot-like cavities, are described in many situations, and the fluted channellings of various rocks are farther adduced, as exhibiting the abrasive power of water.

The 3d class, or the alluvial depositions, offer nothing remarkable. The 4th class, or the native debris, is derived from the disintegration of the subjacent, or primitive rocks. The Nipissing, Lake Huron, &c. offer many examples, the materials composing which appear never to have travelled far, but always to have been derived from the contiguous rocks, being unaltered in their outline and angles.

The paper concludes with the description of a limestone cave

near New Lanark, in Upper Canada, containing bones of various quadrupeds, the larger of which are supposed to have been carried in by a smaller animal, and of the effects of whose *teeth* there are evident marks.

May 19.—A paper was read, entitled, Notes on the Geological Position of some of the Rocks of the N. E. of Ireland; by Lieut. Portlock, Roy. Eng. FGS.

ZOOLOGICAL SOCIETY.

We are happy to be able to announce the complete organization of this Society, the establishment of which has been for some time in contemplation. A meeting of the friends of the Society took place at the Rooms of the Horticultural Society on the 29th April, at which upwards of 100 noblemen and gentlemen were present. Among them we noticed—

The Marquess of Lansdowne, Lords Darnley, Egremont, Gage, Auckland, Stanley, Clinton, the President of the Board of Controul, the President of the Royal Society, the Right Hon. the Lord Mayor, Sir Thomas Dyke Ackland, Sir Robert Inglis, Sir Everard Home, Sir R. C. Fergusson, Sir Stamford Raffles, the Hon. Mr. Twisselton Fiennes, General Thornton, Dr. Goodenough, Mr. W. Hamilton, Mr. H. T. Colebrooke, Mr. Children, of the British Museum, Mr. Duncan, of the Ashmolean Museum, Oxford, Mr. P. Duncan, ditto, Mr. Lambert, Mr. Marsden, Mr. Sotheby, the Rev. Mr. Benson, Mr. Vigors, Dr. Harwood, Dr. Horsfield, Mr. Barnard, Mr. Clift, Mr. Murchison, Capt. De Capel Brooke, Dr. Waring, Mr. Stephens, the Rev. Mr. Rackett, Mr. Haworth, Mr. Griffiths, the Rev. Mr. Hope, &c.

Sir Stamford Raffles having been called to the Chair, a series of resolutions were proposed, and passed unanimously, for the organization of the Society; and the following President, Council, and Officers, appointed:—

President.—Sir Stamford Raffles, President, LL.D. FRS. &c.

His Grace the Duke of Somerset, LL.D. FRS. &c.; Most Noble the Marquess of Lansdowne, FRS. &c.; Right Hon. the Earl of Darnley, FRS. &c.; Right Hon. the Earl of Egremont, FRS. &c.; Right Hon. Viscount Gage, MA. &c.; Right Hon. Lord Auckland; Right Hon. Lord Stanley, MP. VPLS. &c.; Sir Everard Home, Bart. VPRS. &c.; C. Barnard, Esq. FLS. &c.; J. E. Bicheno, Esq. Sec. LS. &c.; J. G. Children, Esq. FRS.; &c.; H. T. Colebrooke, Esq. FRS. &c.; Rev. Dr. Goodenough, FRS. &c.; G. B. Greenough, Esq. FRS. &c.; Major-General Hardwicke, FRS. &c.; Thomas Horsfield, MD. FLS. &c.; Joseph Sabine, Esq. FRS. *Treas.*; Charles Stokes, Esq. FRS. &c.; N. A. Vigors, Esq. MA. FRS. &c. *Sec.*; C. Baring Wall, Esq. MP. &c.

The President then read an opening address to the meeting, explanatory of the past and present state of Zoology in this

country, and of the objects and views of the Society. We hope to have it in our power to present our readers with this address in a future number.

In addition to the members mentioned above as present at the first meeting, the Society already numbers amongst its most zealous supporters the following distinguished personages:—

His Royal Highness the Duke of Sussex, the Dukes of Somerset, Northumberland, and Bedford; the Marquesses of Hertford, Salisbury, and Stafford; Earls Carnarvon, Caledon, Gower, Hardwicke, Lonsdale, Malmsbury, Mountnorris, Minto, Spencer, Stanhope, Winchelsea, Oxford, and Grosvenor; Viscount Dudley and Ward, Viscount Gage; the Bishops of Bath and Wells, London, and Carlisle; Lords Calthorpe, Clifton, Downe, Ducie, Ellenborough, Leveson Gower, Holland, Lovaine, and Selsey; Right Hon. C. Arbuthnot, Right Hon. Sir C. Long, Right Hon. Sir G. Rose, Right Hon. Robert Peele, Right Hon. Sir J. Leach, Right Hon. the Lord Mayor, Right Hon. John Beckett, Right Hon. F. C. Robinson, Hon. Col. Bligh, Hon. G. Agar Ellis, Hon. Capt. Percy, Hon. W. S. Ponsonby, Hon. R. Stopford. Hon. and Rev. Dr. Wellesley, Sir H. Bunbury, Sir C. H. Coote, Sir S. Graham, Sir R. Heron, Sir B. Hobhouse, Sir W. Jardine, Sir J. Shelley, Sir G. T. Staunton, Sir J. Croft, Sir F. Baker, Sir Thomas Lawrence, Sir W. F. Middleton, Sir W. Rawson, Sir P. C. Silvester, Admiral Sir C. Pole, Sir J. E. Smith, Sir H. Halford, John Wilson Croker, Esq. MP. Alexander Baring, Esq. MP. Richard Heber, Esq. the Rev. Dr. Goodall, the Rev. William Kirby, Francis Chantrey, Esq. Alexander Mac Leay, Esq. William Sharpe Mac Leay, Esq. the Dean of Carlisle, &c. E. W. B.

Article X.

SCIENTIFIC NOTICES.

Zoology.

1. *On the Discovery of Live Cockles in a Peat Moss distant from the Sea.*

At the meeting of the Wernerian Society on the 19th of Nov. last, Henry Witham, Esq. read a very interesting paper, "On the Discovery of Live Cockles in Peat-Moss, at a great Distance from the Sea, and much above its present Level." These shells were discovered in the month of October last, in Yorkshire, about forty miles from the sea-coast, in the course of a mineralogical excursion by Mr. Witham through that county. He was led to the spot by a tradition which prevailed in the country of this anomalous occurrence, and found the cockles alive in the sandy bottom of a drain which had been formed through the moss.

This peat-moss is situate about a mile and a half, or two miles (we understood him to say) from Greta Bridge, and about two miles from the river Tees. That cockles had existed in that spot for a period of unknown antiquity is ascertained from the name of the farm in which this peat moss occurs, and which it has borne for centuries–*Cocklesbury*. Specimens of the cockles were laid on the table by Mr. Witham, and of the sand in which they burrowed; and live specimens would have been exhibited, but from the circumstance of the ditch being frozen over when a friend visited the place for the purpose of procuring them. The cockles are found in considerable quantity. Mr. W. gathered a number, and even had the curiosity to eat some of them. They differed but little in taste from the common cockle, unless it were that they seemed not quite so salt.

The specimens of the shells exhibited by Mr. Witham, and of which the writer of this notice, by the kindness of that gentleman, is in possession of one, agree in every respect with those found on most of our sandy shores—the *Cardium edule* of Linnæus. They are of the ordinary size, and nothing in their external appearance would lead any one to suspect they were from a locality so very different. With the exception of one instance, which has been pointed out to us by a scientific friend, nothing similar, as far as has come to our knowledge, has been remarked before; though the publication of Mr. Witham's discovery, by directing attention to the subject, may lead to the knowledge of collateral facts. The instance alluded to is found in the Description of Zetland, by John Brand, published in 1701; and as the statement is interesting, and the book in which it occurs of considerable rarity, we give the passage in the words of the author:

"A gentleman, in the parish of Dunrossness, told one of the ministers in this country, that about five years since, a plough in this parish did cast up fresh cockles, though the place where the plough was going was three-quarters of a mile from the sea; which cockles the gentleman saw made ready and eaten. How these shell fishes came there, and should be fed at such a distance from their ordinary element, I cannot know, if they have not been cast upon land by a violent storm, much of the ground of this parish, especially what they labour, lying very low, and the sea hath been observed in such storms both to cast out stones and fishes; or if these cockles have been found in some deep furrow, from which to the sea there hath been a conveyance by some small stream, upon which the sea hath flowed in stream tides, especially when there is also some storm blowing. If only shells were found, such as of oysters and the like, the marvel would not be great, seeing such are found upon the tops of high mountains, at a greater distance from the sea, which, in all probability, have been there since the universal deluge; but that

any shell-fish should be found at some distance from the sea, and fit for use, is somewhat wonderful and astonishing."*

When Dr. Hibbert was recently in Shetland, he was led by this curious passage to make inquiry on the spot regarding these cockles, but could procure no information on the subject; and the surface of the soil being covered to some depth by drifted sand precluded further investigation.

Prof. Wallace, it may be mentioned, found oyster shells in Bagshot Heath, too recent in appearance to be characterized as fossil, of which the origin is not known;† and modern experiment has proved that shell-fish may be transferred from salt to fresh water with impunity, though it is difficult to believe that the ingenuity of our ancestors exerted itself in providing such articles of luxury in such a way. The fact observed and related by Mr. Witham, therefore, of live cockles being found at a distance so considerable from the sea, and at such a height above its level, can only be accounted for by the retrocession of the ocean—or by supposing some great convulsion to have submerged the land, and left these evidences of its effects. In any view, the discovery is interesting, and similar occurrences will probably lead to a modification of the prevailing theories. If the shells in question had not been found alive, it might have been conjectured that they had been deposited there at a very distant period, by one of those catastrophes which are supposed to have changed the bed of the ocean, or floated its astonished inhabitants over the land, and an unknown and mysterious antiquity thus assigned to shells which might have been alive shortly before. That similar circumstances have, on more occasions than one, misled observers, we have little doubt. We have seen specimens of shells from the banks of Lochlomond, which seem, from their appearance, to be in this predicament; and instead of supposing that these were the remains of animals which had been left there when Lochlomond joined the eastern and western seas, we should conjecture that they had recently lived and died in the very lake on the banks of which they were found.

In the paper, by Mr. J. Adamson, which gave an account of the shells thus found, and which is printed in the Wernerian Transactions, vol. iv. p. 334, that gentleman says, that "the shells begin to appear about half-way between the highest and lowest, or the winter and summer surfaces of the water, which

* A Brief Description of Orkney, Zetland, Pightland-Firth, and Caithness, &c. By John Brand, pp. 115, 116. Edinburgh, 1701.

† [Is it not more probable that these oysters were fossils belonging to the Upper Marine Formation, of which the Bagshot sand forms a member? For although Mr. Warburton states that the shelly matter of the fossil shells which certain beds in the Bagshot sand contain has altogether perished, yet in the corresponding deposits of the crag in Suffolk, and the upper marine formation in the Isle of Wight, many of the shells can scarcely be distinguished from recent ones. All these deposits, it will be remembered, agree with the upper marine formation of the Paris basin.—E. W. B.]

varies in this respect about six feet. After removing a slight covering of coarse gravel, we find a thin bed of clay, of different shades of brown, passing into yellow colours, as we descend. In the *upper*, or brown clay, are found shells of the following species:—Those marked with an asterisk are doubtful. Buccinum reticulatum,* Nerita glaucina, Tellina tenuis,* Cardium edule, Venus striatula, Venus Islandica, Nucula rostrata, young, Pecten obsoletus, Anomia Ephippium, young, Balanus communis, Balanus rugosus, Echinus esculentus.

"A skilful conchologist would discover many others, from the numerous traces of them in the clay. Those shells appear to have been deposited generally in an entire state, and many are found with both valves in their natural position. The Balanus is still slightly attached to the Venus or Pecten; and the spines of the Echinus are found clustered in the clay inclosing its fragments; so that they must have been either covered by water to a considerable depth, or thrown on a beach not much exposed to waves. Few of them, however, can be extracted entire, as several of the species are always in a state of gritty chalk; but many complete and beautiful specimens of the Pecten can easily be procured. Few of their fragments appear on the exposed part of the beach, but, during summer, many may be seen a few feet under water."

We lately received several specimens of the *Buccinum Lapillus* from Shetland, which were found alive on the margin of a lake in the island of Yell, about a mile and a half from the sea. The lake has an outlet by a small rivulet. The shells are somewhat thinner in their texture than their congeners on the rocks of the neighbouring coast, and are all of the banded variety of that shell, or crossed with dark-coloured lines. That these shells had been carried to that locality by water-fowl is not unlikely; and the outer lip of the shells being somewhat broken, may have occurred in the attempt to extract the animal as food. But the fact of the animals being alive when the specimens were picked up, goes to prove that shell-fish may be brought to live in fresh-water; and the experiments undertaken by Mr. Arnold, of Guernsey, at the suggestion of Dr. Mac Culloch, and the discovery of live cockles at a distance from the sea by Mr. Witham, leave little room to doubt that many species of fish may be transported to, and live and propagate in, inland fresh-water ponds and rivulets.—(Edin. Journ. of Science.)

MISCELLANEOUS.

2. *Query respecting Sound, said to be produced by the Rupture of the Tendo Achillis.*

GENTLEMEN, *April* 26, 1826.

Mr. Stanley, in an Anatomical lecture delivered in the Theatre

at St. Bartholomew's Hospital on the 1st of March, said, when speaking of the rupture of the tendo achillis, "it sometimes happens when a person is dancing, and so loud a noise is produced by it, as to be distinctly heard at the other end of the room. This may be urged as an argument in favour of the opinion, that *sound is a distinct substance,* and not the result of *the vibrations of the air,* as some have supposed; for here is sound produced in a situation to which the air has not access."

Not being able to reconcile this observation with what I understand to be the opinion entertained by modern philosophers, I take the liberty of addressing you on the subject; and if through the medium of your valuable journal, you will inform me how far Mr. Stanley's notion be correct, it will oblige,

Gentlemen, yours, &c.

A BARTHOLOMEW'S PUPIL.

*** We are quite as much at a loss to comprehend Mr. Stanley's doctrine (if it be correctly stated) as our correspondent. We publish the *Bartholomew's Pupil's* letter, therefore, that Mr. S. may, if he please, enter more fully into an explanation of his views concerning sound, in our next, or any future number of the *Annals of Philosophy.—Ed.*

3. *Phosphorescent Plants.*

Several cryptogamous subterraneous plants have been observed to be luminous in the dark. M. Nees, of Esenbeck, cites after M. Heinzmann, the *rhizomorpha phosphorescens* found in the mines of Hesse, in the north of Germany: the light is visible at the extremities of the plant, especially when it is broken. This phosphorescence disappears in hydrogen gas, oxide of carbon, and chlorine gas. Some other *rhizomorpha,* as the *subterranea* and the *aïdula,* have also appeared phosphorescent to several persons working in the mines.—(Journal de Pharmacie.)

4. *Raining Trees.*

In the ancient histories of travellers in America, and also by Thévet in his Cosmographia, mention is made of a tree which attracted the clouds from the heavens, and converted them into rain in the dry deserts. These relations have been considered as fables. There has been lately found in Brazil a tree, the young branches of which drop water, which fall almost like a shower. This tree, to which Leander has given the name of *cubea pluviosa,* is transferred by M. Decandolle to the genus *Cæsalpinia* (pluviosa), in his *Prodomus,* vol. ii. p. 483. Also many vegetables, as the *calamus rotang,* and tender climbing plants, the vine, and other twigs, at the season of sap, particularly when they are cut, *weep* abundantly. This genus Cæsalpinia, which furnishes the dyeing wood of Fernambuco

and Sappan, presents also a species, the leaves of which are almost as sensible to the touch as the sensitive plants at Malabar; it is the *Cæsalpinia mimosoides,* Lamarck.—(Journ. de Pharm.)

5. *On the Poisoning of Plants.*

Plants are liable to lose, as Carradori has seen, by distilled oleander water, their power of contraction; thus this water, or, even better, the volatile oil of oleander, extinguishes all the power of contraction of the capsule of *momoidica elaterium,* and of *balsamina impatiens.* Mr. Marcet, of Geneva, having soaked sensitive and other plants in an aqueous solution of opium, remarked that it also extinguished the action of vegetable life. Thence Carradori concludes, that plants have contractable muscular fibres. Mr. Marcet has thought that vegetables also possess something analogous to a nervous system, since the first of these poisons acts on contraction, the second on the sensibility in animals.—(Journ. de Pharm.)

6. *Distribution of Land and Water.*

From the unequal distribution of the continents and seas, the southern hemisphere has long been represented as eminently aquatic; but the same inequality makes its appearance, when we consider the globe divided, not in the direction of the equator, but in that of the meridians. The great masses of land are collected between the meridians of 10° to the west, and 150° to the east of Paris; while the peculiarly aquatic hemisphere commences to the westward with the meridian of the coasts of Greenland, and terminates to the east with the meridian of the eastern shores of New Holland and the Kurile Isles. This unequal distribution of the land and water exercises the greatest influence upon the distribution of heat at the surface of the globe, upon the inflexions of the isothermal lines, and upon the phenomena of climate in general. With reference to the inhabitants of the centre of Europe, the aquatic hemisphere may be called western, and the terrestrial hemisphere eastern; because in proceeding westward, we come sooner to the former than to the latter. Until the end of the 15th century, the western hemisphere was as little known to the inhabitants of the eastern hemisphere as one-half of the lunar globe is at present, and probably will always remain to us. *Humboldt.*—(Edin. Phil. Jour.)

7. *Corrigenda.*

M. Del Rio has had the goodness to point out one or two errors in the translation of his paper, " On the Analysis of an Alloy of Gold and Rhodium, from the Parting House at Mexico,* which we are anxious to correct. *For* " from the

* *Annals*, vol. x. New Series, October, 1825.

fused silver ingots," p. 254, l. 7 and 8, *read* "from the silver ingots obtained by fusion." *For* "as if we employed more than one (acid), and they were very volatile and easily decomposed," p. 255, l. 16 and 17 from bottom, *read* "as if more than one acid were used, and that very volatile and easily decomposed." *For* "worth 1800 piastres," p. 256, l. 12, *read* "the sum of 1800 piastres."—*Ed.*

Article XI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

The Hunterian Oration, delivered at the Royal College of Surgeons, by Sir Anthony Carlisle, containing the Natural History of the Oyster, and some of the principal points in its Anatomy.

A Letter to Mr. Thomas Brown, Surgeon, Musselburgh, containing Remarks on his "Letter to the Right Hon. the Earl of Liverpool, concerning the present State of Vaccination." By Henry Edmondston, MA. Surgeon, Newcastle-upon-Tyne.

A Treatise on the Nature and Cure of Rheumatism, with practical Remarks on Neuralgia, or painful Affection of Nerves. By Charles Scudamore, MD.

Mr. H. W. Dewhurst has in the Press a Dictionary of Anatomy and Physiology; also another work to be entitled, Synoptical Tables of the Materia Medica.

JUST PUBLISHED.

The Second Volume of Dr. Bostock's Elementary System of Physiology. 8vo. 16*s.*

Travels in Chile and La Plata, including Accounts respecting the Geography, Geology, Statistics, &c. and the Mining Operations in Chile. By John Miers. 2 vols. 8vo. with Maps and Plates.

The Surgeon Dentist's Anatomical and Physiological Manual. By G. Waite. 5*s.* 6*d.*

Practical Botany, an improved Arrangement of British Plants, with a familiar Introduction to the Linnæan System. By W. Johns, MD. FLS. &c. Post 8vo. 9*s.*

Gleanings of Chemistry, illustrating the Nature of Air, Water, Light, Heat, &c. Post 8vo. 6*s.* 6*d.*

Historical Researches on the Wars and Sports of the Mongols and Romans, in which Elephants and Wild Beasts were employed or slain, and the remarkable local Agreement of History with the Remains of such Animals found in Europe and Siberia. By John Ranking, Resident upwards of Twenty Years in Hindostan and Russia. 4to. With Map and Plates. 3*l.* 3*s.*

No. 8, of the Zoological Journal, with No. 2, of the Supplementary Plates: conducted by Thomas Bell, Esq. FLS.; J. G. Children, Esq. FR. and LS.; J. D. C. Sowerby, Esq. FLS.; and G. B. Sowerby, FLS.

Article XII.

NEW PATENTS.

J. Billingham, Norfolk-street, Strand, civil engineer, for an improvement or improvements in the construction of cooking apparatus.—April 18.

J. Rowbotham, Great Surrey-street, Blackfriars-road, hat manufacturer, and R. Lloyd, Strand, for a certain method of preparing, uniting, combining, and putting together, certain materials, substances, or things, for the purpose of being made into hats, caps, bonnets, cloaks, coats, trowsers, and for wearing apparel in general, and various other purposes.—April 18.

W. Wood, Summer Hill Grove, Northumberland, for an apparatus for destroying the inflammable air in mines.—April 22.

J. P. Gillespie, Grosvenor-street, Newington, for a new spring or combination of springs, for the purpose of forming an elastic resisting medium.—April 25.

S. Brown, Eagle Lodge, Old Brompton, for improvements on an engine for effecting a vacuum, and thus producing powers by which water may be raised, and machinery put in motion.—April 25.

F. Halliday, Ham, Surrey, for an apparatus for preventing the inconvenience arising from smoke in chimneys.—April 25.

J. Williams, Commercial-road, ironmonger and ship's fire hearth manufacturer, for improvements on ships' hearths, and apparatus for cooking by steam.—April 27.

W. Choice, Strahan Terrace, auctioneer, and R. Gibson, White Conduit Terrace, builder, Islington, for improvements in machinery for making bricks.—April 27.

C. Kennedy, Virginia Terrace, Great Dover-road, Surrey, surgeon and apothecary, for improvements in the apparatus used for cupping.—April 29.

J. Goulding, Cornhill, London, engineer, for improvements in the machines used for carding, stubbing, slivering, roving, or spinning wool, cotton, waste, silk, short stapled hemp and flax, or any other fibrous materials, or mixture thereof.—May 2.

A. Buffurn, Javin-street, hat-manufacturer, and J. M'Cardy, Cecil-street, Strand, for improvements in steam engines.—May 6.

Sir R. Seppings, Somerset House, for improvements in the construction of fids or apparatus for striking top-masts and top-gallant masts in ships.—May 6.

W. Fenner, Bushell Rents, Wapping, carpenter, for an improvement in machinery for curing smoky and cleansing foul chimneys.—May 6.

A. Allard de la Court, Great Winchester-street, for a new instrument, and improvements in certain well-known instruments applicable to the organ of sight.—May 6.

J. Schaller, Regent-street, ladies' shoe-maker, for improvements in the construction or manufacture of clogs, pattens, or substitutes for the same.—May 6.

E. Heard, St. Leonard, Shoreditch, chemist, for a certain new composition to be used for the purpose of washing in sea and other water.—May 8.

Article XIII.

Extracts from the Meteorological Journal kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Mr. E. C. Giddy, Curator.

1826.	Barometer.			Regist. Therm.			Rain in 100 of inches.	Wind.	Remarks.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Apr. 23	29·79	29·70	29·745	50	44	470		NW	Fair, clear.
24	29·88	29·80	29·840	50	44	470		N	Fair, clear, a shower.
25	29·90	29·86	29·880	54	48	510	0·09	NW	Fair, rain.
26	29·70	29·68	29·690	56	52	540		NW	Fair.
27	29·60	29·52	29·560	56	40	480		NW	Fair, clear.
28	29·94	29·90	29·920	48	40	440		NW	Clear, a hail shower.
29	29·98	29·98	29·980	50	40	450		N	Clear.
30	30·02	29·98	30·000	50	38	440		E	Clear.
May 1	30·12	30·12	30·120	55	38	465		NE	Clear, frost.
2	30·00	29·94	29·970	57	46	515		NW	Fair, clear.
3	29·90	29·90	29·900	57	48	525		NE	Clear.
4	29·98	29·96	29·970	50	45	475		NE	Clear.
5	29·98	29·95	29·965	54	40	470		N	Cloudy.
6	29·98	29·98	29·980	54	43	485		NE	Cloudy, clear.
7	29·98	29·96	29·970	52	40	460		E	Clear.
8	29·90	29·86	29·880	58	45	515		SE	Cloudy.
9	29·90	29·90	29·900	60	45	525		Var.	Clear.
10	29·90	29·90	29·900	59	45	520		NW	Clear.
11	29·95	29·90	29·925	58	45	515		NW	Clear.
12	30·00	30·00	30·000	59	48	535		E	Clear.
13	30·02	29·94	29·980	58	43	505		E	Clear.
14	29·92	29·92	29·920	61	45	530		E	Clear.
15	29·98	29·95	29·965	62	50	560		E	Clear.
16	30·00	30·00	30·000	64	50	570		NW	Fair, clear.
17	30·00	29·99	29·995	65	50	575		NW	Clear.
18	29·96	29·80	29·880	66	50	580		SW	Clear.
19	29·74	29·54	29·640	60	52	560	0·20	SE	Cloudy, rain.
20	29·60	29·54	29·570	62	48	550		W	Showers, clear.
21	29·90	29·88	29·890	62	50	560		N	Clear.
22	29·92	29·90	29·910	69	52	605		N	Clear.
	30·12	29·52	29·888	69	38	520	0·29	NW	

RESULTS.

Barometer, mean height .... 29·888

Register Thermometer, ditto .... 520°

Rain, No. 1, 0·290, No. 2, 0·450.

Prevailing wind, NW.

No. 1. This rain guage is fixed on the top of the Museum of the Royal Geological Society of Cornwall, 45 feet above the ground, and 143 above the level of the sea. No. 2. Close to the ground, 90 feet above the level of the sea.

Penzance, May 23, 1826. EDWARD C. GIDDY.

ARTICLE XIV.

METEOROLOGIGAL TABLE.

1826.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
4th Mon.							
April 1	S W	30·52	30·31	54	41	—	
2	N W	30·31	30·30	54	50	—	
3	N W	30·32	30·30	62	47	—	
4	N W	30·30	30·27	60	48	—	
5	N W	30·28	30·24	54	47	—	
6	N W	30·26	30·24	60	48	·47	
7	N W	30·29	30·26	66	46	—	
8	N W	30·29	29·94	69	47	—	
9	S	30·08	29·94	68	43	—	03
10	W	30·08	29·99	60	48	—	—
11	W	29·99	29·27	62	46	·48	45
12	N W	30·27	29·27	54	40	—	33
13	N W	30·40	30·27	62	48	—	
14	N W	30·40	30·40	63	46	—	
15	N W	30·40	30·31	70	49	·49	—
16	N W	30·43	30·31	64	36	—	
17	N W	30·43	30·41	60	28	—	
18	S E	30·41	30·27	64	38	—	
19	S E	30·27	30·01	64	31	—	
20	E	30·01	29·87	65	38	—	
21	S E	29·87	29·80	70	48	—	
22	S E	29·93	29·81	70	45	·95	—
23	N W	30·04	29·93	64	30	—	
24	N W	30·16	30·04	58	31	—	
25	N W	30·16	29·87	56	38	—	15
26	N W	29·87	29·67	60	38	—	11
27	Var.	30·05	29·66	48	31	—	05
28	N W	30·17	30·05	46	31	—	
29	N W	30·32	30·17	53	25	—	—
30	N	30·43	30·32	58	30	·82	
		30·52	29·27	70	25	3·21	1·12

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fourth Month.—1. Fine. 2. Cloudy. 3—8. Fine. 9. Fine: a shower of rain about five, p. m. 10. Fine. 11. Showery night. 12. Rainy. 13—17. Fine. 18. Fine: a very distinct lunar halo. 19. Ditto. 20—24. Fine. 25. Showers. 26. Fine: some rain in the night. 27. Showers. 28. Fine. 29. Slight showers of hail during the day. 30. Fine.

RESULTS.

Winds: N, 1; E, 1; SE, 4; S, 1; SW, 1; W, 2; NW, 19; Var. 1.

Barometer: Mean height

For the month.................................... 30·144 inches.

Thermometer: Mean height

For the month.................................... 50·500°

Evaporation.................................... 3·21 in.

Rain.................................... 1·12

Laboratory, Stratford, Fifth Month, 20, 1826. R. HOWARD.

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